

UK-HQ 6223-23-P35433

THE
BRITISH JOURNAL
OF
PSYCHOLOGY

GENERAL SECTION

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VOLUME XXIV, 1933-34

CAMBRIDGE
AT THE UNIVERSITY PRESS

1934

23

P. 35433

PRINTED IN GREAT BRITAIN

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A STUDY IN THE INHERITANCE OF
INTELLIGENCE.THE ANALYSIS OF 100 FAMILIES CONTAINING
SUBCULTURAL MENTAL DEFECTIVES.

BY L. S. PENROSE.

- I. *Introduction* (pp. 1-3).
- II. *Size of the family and order of birth* (pp. 3-4).
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I. INTRODUCTION.

SINCE Goddard (1) and Davenport and Weekes (2) published their analyses of families containing mental defectives there have been numerous attempts to discover genetic causes of mental deficiency by studying pedigrees (3). In a recent investigation Sjögren (4) has attempted to analyse the heredity of a fairly large group of cases with similar characteristics. The subject is beset by many difficulties, one of the most prominent of these being that there are such a great number of different types of mental defect that it is not easy to collect together for study a large group of cases which are definitely of one single type. Any inferences about the genetics of mental deficiency made without subdivision into the different types must necessarily be of very limited value.

In the present instance the data are derived from a hundred families which have been selected by the presence of at least one son or daughter who is mentally defective. The cases, through whom the families were selected, are all patients in the Royal Eastern Counties' Institution, and they were carefully chosen so as to exclude all individuals in whom

secondary causes of mental deficiency might be supposed to have been operative. In addition to this, all specific types of defectives were excluded, such as mongols, microcephalics, epileptics, psychotics, cretins and neurological cases. Also those families were excluded in which congenital syphilis was suspected. The residual group of patients that is left may be termed the subcultural type. This type is not really a single entity: it is a collection of persons who are healthy, apparently sound neurologically and not suffering from any definite psychosis, but who lack intelligence. According to E. O. Lewis⁽⁵⁾ their mental measure is to be regarded as forming the lower part of the normal frequency distribution curve of intelligence.

The families of these patients were, in every case, personally visited, often several times. The parents were interviewed and other relatives also visited if necessary. Official reports made by the local Voluntary Associations were consulted in every case. Relieving officers, clergymen and doctors were applied to for information in a great many instances.

The study described in the present paper is to be regarded as merely preliminary to more intensive investigation of greater numbers. It has one serious drawback, namely, that the intelligence of the parents and sibs of defectives, in the great majority of cases, was only assessed roughly and not measured by standardized tests. The average I.Q. of 103 mentally defective persons in these families, who were actually tested by standardized tests, was 49.9 ± 18.7 . The Stanford-Binet scale was used and the divisor for adults was taken to be 14 years. The mentality of the remaining members of the families was judged by personal interview, the opinions of their friends, schoolmasters' and doctors' reports, as well as by consideration of general social ability. The following four classes were used, and the total number of persons (parents and children) classified in this way was 595:

Arbitrary class	Approximate I.Q.
1. Superior intelligence	125
2. Normal intelligence	100
3. Dull	75
4. Mentally defective	50

Naturally a large number of children were of unknown mentality, but only families with parents of known mentality were included. As far as possible the standards were the same for parents and children. The precise relationship, however, between intelligence of parents and children, in such a study as this, cannot be ascertained until either there are standardized intelligence tests available for adults or until children already tested have grown up and had offspring. It must be remembered throughout, in the consideration of the subsequent figures, that these classes are only

a makeshift. They grade into one another and probably quite often overlap, but they give a general idea of the relationship of intelligence of children to their parents and to one another.

An analysis of facts discovered in studying the 100 families now follows, the object of which is to obtain information concerning the parts played by environment and heredity in determining intelligence.

II. SIZE OF THE FAMILY AND ORDER OF BIRTH.

It has often been suggested that the size of the family is inversely proportional to the intelligence of the parents and a great deal has been said about the disproportionate propagation of persons of a low mentality. The crude average size of the family discovered here, judged by the number of pregnancies, is 6.0 and would suggest the possible truth of these suppositions. On the other hand, the criticism which Greenwood and Yule(6) applied to Pearson's data concerning tuberculous families, and other similar data, applies to this crude figure. The correct estimate of the size of the family can only be obtained by taking into consideration the fact that large families are more likely to be noticed when selection is being made by the presence of at least one affected member. The average size of the family, when corrected by Yule's method, is the total number of children divided by the expected proportion of first born, as calculated in the "place in family" table (see Table I). The value is $150/39.9$, i.e. 3.7, which is certainly by no means abnormally large, seeing that it includes nearly 10 per cent. of miscarriages.

Table I.

Order of birth	Size of sibship											Total	Expecta- tion
	1	2	3	4	5	6	7	8	9	10	11 +		
1	15	2	5	3	4	3	1	.	1	.	1	35	39.9
2	.	4	7	5	5	2	.	1	.	1	3	28	24.9
3	.	.	4	3	2	7	2	.	1	.	2	21	21.9
4	.	.	.	5	2	3	1	2	.	1	3	17	16.6
5	.	.	.	.	2	3	1	.	2	1	2	11	12.6
6	.	.	.	.	.	1	2	1	1	1	2	8	9.6
7	.	.	.	.	.	.	2	2	1	.	1	6	6.4
8	.	.	.	.	.	.	.	3	1	2	.	6	5.1
9	.	.	.	.	.	.	.	.	1	1	4	6	4.0
10	.	.	.	.	.	.	.	.	.	2	3	5	3.1
11 +	.	.	.	.	.	.	.	.	.	.	7	7	5.8
Total	15	6	16	16	15	19	9	9	8	9	28	150	149.9

The expectation is calculated as follows:

If N_1 be the number of only children, N_x the number of affected children in sibships of 2, N_x the number of affected children in sibships of x , the expected number of r th born children is $\sum_r \left(\frac{N_x}{x} \right)$, where k is the size of the largest sibship.

A Study in the Inheritance of Intelligence

Shrubsall (7) showed, by the analysis of the order of birth of a thousand mentally defective children, that defective offspring were more likely to come late in the family than early. The present data show a similar effect. In the whole series of families there seems to be a slight tendency for the less intelligent children to be born late. An important check on this conclusion is obtained by comparing the different proportions of normal and affected children born at different maternal ages. This comparison has been made, and the result is shown in Table II. All pregnancies have been counted in 94 families where the maternal age was known accurately. A slight, but definite, rise of the proportion of defective children accompanies increasing maternal age: the significance of this effect is, however, somewhat diminished by the varying numbers of pregnancies of unknown mental outcome in different maternal age groups. Even allowing for this, though, the children born before the maternal age of 25 have a distinct advantage in mental ability over those born after the age of 35.

Table II.

Mentality of Children	Maternal age at birth of offspring (94 families).							
	Below 25 years		25-29 years		30-34 years		35 years and above	
	%		%		%		%	
Normal	50	42	51	33	39	29	51	31
Dull	10	8	12	8	13	10	20	12
Mentally defective.	23	19	34	22	36	26	46	28
Unknown	36	30	56	37	48	35	47	29
Total	119		153		136		164	

III. HOME CONDITIONS.

As might be expected, many of the homes of the families which produce subcultural mental defectives are of a low social grade. One would not imagine that mentally defective parents would take much trouble to keep their homes very clean, and poverty may be the result of low intelligence as well as a possible cause of it. Two scales of rating were used to describe the home conditions, (a) the number of rooms per person living in the home, and (b) a classification of general management based on estimates of cleanliness, order and comfort. For the latter classification the following five standards were used:

- (A) Very good.
- (B) Good average.
- (C) Average.
- (D) Poor average.
- (E) Bad.

No homes suitable to be classed under (A) were discovered, but this is not unnatural, since institutional life is unnecessary for defectives living under the best conditions.

(a) The distribution of the criterion of rooms per person among the families with different types of parents is shown in Table III.

Table III.			
No. of families	Type	Less than one room per person	One or more rooms per person
34	Parents both normal	11	23
6	Parents both defective	4	2
53	Father normal	19	34
21	Father dull	9	12
8	Father mentally defective	4	4
41	Mother normal	16	25
19	Mother dull	7	12
22	Mother mentally defective	9	13

The average number of rooms per person in these families works out at 1.22, a figure intermediate between figures found for areas from which most of the patients are drawn. The 1921 Census gave figures of 1.09 for Essex and 1.29-1.34 for Suffolk. Although there is little indication of these families being abnormal socially, judged by this criterion, we must not forget that many of them may have had their home conditions considerably improved as a result of the attention given to them by social workers and local authorities. More important here than the absolute number of rooms per person is the relative tendency to overcrowding which occurs when the parents are of low mentality. The size of the home is, moreover, closely related to the mentality of the father and hardly dependent at all on the mentality of the mother. Since the criterion of rooms per person is an economic one, this result is to be expected, as the father is usually the wage earner.

(b) The grouping of different types of home for different types of parents also showed the importance of parental intelligence. If the standards (A) to (E) are given the numerical values of 4 to 0, the average markings for different types of families are as shown in Table IV.

Table IV.		
Type of family	No. of families	Average marking for home conditions
Theoretical average family	—	2.00
Parents both normal	41	1.83
Intermediate parental grades	43	0.88
Parents both mentally defective	7	0.43

The correlation between mentality of father and home conditions was 0.25—slightly below that between mother and home conditions, which was 0.32. This result, though the converse of that obtained for the rooms per person criterion, is also to be expected. The management of the home depends more on the mother's mentality than on the father's. These two criteria, although only giving results which would be given by the application of common sense, are useful in that they check the estimate of mentality of the parents which has been made. Had the results of these home condition investigations turned out otherwise, one would have suspected that there was something wrong in the estimates of parental mentality.

A correlation of 0.17 was found for intelligence of sons and home conditions, and the corresponding value for daughters was 0.22. By the uncritical application of statistical formulae to eliminate the effect of parents on home conditions and on the mentality of children, partial correlations of the order of 0.09 are found for intelligence of children and home conditions. It would, however, be rash to infer from this low value, without much further enquiry, that the effect of varying home conditions on the intelligence of children is insignificant.

IV. THE PROPORTION OF DEFECTIVES IN FAMILIES BORN TO DIFFERENT TYPES OF PARENTS.

Table V summarizes the data collected concerning the relative mentality of parents and children. The crude statement of the proportion of defectives found in different classes of families, gathered directly from the table, has little immediate significance because each family contains at least one defective. But it is clear from casual observation of the number of normal children born to different types of parents that parental mentality and filial mentality are closely related to one another. It is equally clear that no simple hypothesis of Mendelian inheritance is going to describe the results with accuracy. For example, a single dominant gene is ruled out by the fact that so many defectives have normal parents. Further, the very low incidence of defectives in families where both parents are normal rules out a single recessive gene. Among the 143 brothers and sisters of patients whose parents were both normal, only 4 were defective and 13 dull, and these were, to some extent, counterbalanced by 7 children who had won scholarships for secondary schools.

The simplest explanation accounting approximately for these figures on Mendelian lines is the assumption of an intermediate type of inheritance

Table V. *Analysis of 100 families containing mental defectives of the subcultural type.*

Father	Normal		Normal		Normal	
Mother	Normal		Dull		Mentally defective	
No. of families	45		10		12	
		Expected		Expected		
Children	Observed	($p=0$)	Observed	($p=0$)	Observed	($p=0$)
Superior	7	(0)	—	—	—	—
Normal	119	(143)	17	(16.5)	11	(0)
Dull	13	(0)	13	(16.5)	14	(33)
Defective	49	(45)	13	(10.0)	20	(12)
Total	188		43		45	

Father	Dull		Dull		Dull	
Mother	Normal		Dull		Mentally defective	
No. of families	8		9		7	
		Expected		Expected		Expected
Children	Observed	($p=0$)	Observed	($p=\frac{1}{2}$)	Observed	($p=\frac{1}{2}$)
Superior	—	—	—	—	—	—
Normal	19	(14.5)	8	(7.8)	—	(0.0)
Dull	7	(14.5)	12	(15.6)	1	(7.3)
Defective	11	(8.0)	17	(13.6)	18	(11.7)
Total	37		37		19	

Father	Mentally defective	Mentally defective	Mentally defective
Mother	Normal	Dull	Mentally defective
No. of families	0	2	7
	Expected	Expected	Expected
Children	Observed ($p=0$)	Observed ($p=\frac{1}{2}$)	Observed ($p=1$)
Superior	—	—	—
Normal	—	(0.0)	1 (0)
Dull	—	(1.3)	3 (0)
Defective	—	4 (2.7)	18 (22)
Total	—	4	22

Total children	Observed	Expected
Superior	7	(0.0)
Normal	175	(181.9)
Dull	63	(90.0)
Defective	150	(123.1)

of mental defect in which the heterozygote or "carrier" is recognizable as a dull person. The genotypes would then be as follows:

Normal : AA
 Dull : AB or BA
 Mentally defective: BB .

Expectations can be worked out on this assumption by the application of any method which allows for the circumstance that at least one

member in each fraternity is affected. The most satisfactory way of doing this is by use of the following formula for the expected number of affected offspring in such families(8).

Where p is the probability that an offspring is affected in a given family and n_s is the number of s -membered fraternities each containing at least one affected member, the expected number of affected persons, termed r_s , has the value

$$p \cdot \Sigma n_s \cdot \frac{s}{1 - (1 - p)^s}.$$

The expected relative proportions of the remaining members of the fraternity, on whatever genetic hypothesis is chosen, will be unaltered, though their total number is relatively diminished. Thus, in the case of parents both heterozygous, $\frac{1}{4}\Sigma n_s \cdot \frac{s}{1 - (1 - \frac{1}{4})^s}$ of the children will be affected, two-thirds of the remainder heterozygous and one-third of the remainder normal, the proportion of heterozygous to normal being still 2:1. In the limiting case where $p \rightarrow 0$, the incidence of affected offspring approaches nil, and the above formula approaches the limiting value $\Sigma n_s \cdot 1/s$ for the expected number of affected persons which equals the total number of families observed—i.e. in each family selected, only one should be affected.

The values of the expectations obtained by this method are given in brackets after the observed totals in Table V. The comparison of the crude figures with these expectations reveals a general approximate, but nowhere exact, agreement. Taking the totals of normal and superior together, the resulting value is very close to the expectation for normal, and, similarly, the sum of the dull and defective children is close to the sum of their expectations. When we remember how difficult it is to distinguish between dull and defective without actual testing and, indeed, even with the aid of tests, this discrepancy between expectation and observation seems less important. On the other hand there is a quite definite scattering of observed values on either side of the expected values. A scattering of this kind, which retains a general tendency to agreement with these expected values, could be produced if the condition which was being studied were the result of a number of alternative additive factors. This supposition has been used by Fisher(9) to explain the inheritance of physical stature. Mental stature may be subject to the same kind of additive multifactorial inheritance.

V. THE STATUS OF PARENTS IN RESPECT OF INTELLIGENCE.

In the present series of families quite a considerable number of parents are defective and in seven cases both parents are of this grade. It cannot be too strongly emphasized that this percentage is much higher than would be found if mental defectives of all types had been grouped together. The high percentage of defective parents is characteristic of this particular class of patients, and nearly all the mental defectives of other types have normal parents. Furthermore, it must be remembered that defective parents have a much greater proportion of their offspring affected than normal parents and they are, therefore, very much more likely to come under observation than normal parents. The figures have, therefore, no direct relation to the actual frequency of such unions. On the other hand, there is strong evidence that unions between parents of like mentality are more frequent than they should be if mating between parents of different mental grades took place entirely at random. The correlation table connecting the mentality of parents with one another is as follows:

		Father		
		Normal	Dull	Mentally defective
Mother {	Normal	45	8	0
	Dull	10	9	2
	Mentally defective	12	7	7

The value of r is $+0.44$, showing that a very considerable degree of likeness exists between father and mother in respect of mental grade. In particular, there is no example of a mentally defective father mating with a normal mother. Either for psychological or economic reasons, such unions are rare. The complementary type of union, where a defective woman has a husband of normal intelligence, is comparatively common. One often finds, however, that the father in such families is of neurotic character or otherwise mentally deranged: but whether this is the cause or the effect of the union is uncertain.

The interest of the inter-parental positive correlation does not cease here, for, in evaluating the importance of likenesses found between parents and children and between siblings themselves, assortative mating between parents is of considerable significance. Supposing the frequencies of the different types of parents are subject to a modification, by like persons mating with like, to the extent that the correlation between parents is h in respect of a given quality, it can easily be shown algebraically that for conditions where the heterozygote is intermediate, the

correlation between parent and child should be $\frac{1+h}{\sqrt{2(2+h)}}$, and the inter-correlation between siblings should be $\frac{1+h}{2+h}$. The limiting values of these expressions, when $h = 1$, are respectively 0.82 and 0.67. Thus when a parent-child or fraternal correlation in a group of persons is found to be greater than 0.50, it is not wise to assume, as has sometimes been done, that this effect is due to environmental influence causing increased likeness. It may be due to inter-parental correlation. It is quite possible that a fairly strong degree of likeness of intelligence may exist among normal parents in the general population. For instance, when Thorndike (10) found that the correlation for intelligence of siblings in American families was 0.60, he considered it indicative of environmental effects. But, since Willoughby (11) demonstrated that an average correlation of 0.44 existed between parents for mental tests and higher correlations have been found by Jones (12), one must accept 0.60 as a very likely value for the correlation between sibs of mental characters due entirely to heredity.

In the present instance the crude parent-child correlation, found by simply taking all pairs of parents and children, regardless of sex, works out at 0.36. But we cannot conclude that this is even in approximate agreement with the value of 0.50, given by Fisher and Pearson, which would be expected if the likeness were hereditarily determined. With an inter-parental correlation of 0.44, the expected value of 0.50 for the parent-child relation must be raised to 0.65. It will be seen that the value obtained in the present investigation falls considerably short of expectation. Such a discrepancy is the kind of effect which would be produced if environmental—'secondary'—causes of mental deficiency were operating to make children *less* like the parents than they would be if heredity alone were responsible.

A methodological source of error in interpreting here the observed value of the correlation between parents and offspring is the circumstance that only families with at least one affected child are included. The correction for this factor tends to lower the expected correlation, but no accurate new value can be assigned since the expectation in this contingency is a function of the gene frequency as well as of the size of the family. We can be confident, however, that this consideration counterbalances, to a considerable extent, the raising of the expected correlation by the observed assortative mating of the parents.

VI. EXAMINATION FOR SEX-LINKED INHERITANCE.

On reference to the analytical table which summarizes the number of children born to different types of parents, it will be found that the excess of defectives above the expectations given is somewhat greater when the mother is of low mentality than when the father is of low mentality. Thus it looks as though the association of the mentality of the offspring with that of the mother was greater than with that of the father. A similar effect was noticed by Goddard (1), pp. 553-4) and examined critically but unconvincingly by Rosanoff (13). In order to find whether it may be due to the operation of sex-linked factors, the sexes must be sorted out and compared with the parents separately. In the present series of families (see Appendix, p. 16), the crude correlations—taking simply all pairs of parents and children in each group—have the observed values which are shown in Table VI.

Table VI. *Parent-child correlation values (heterozygote intermediate)*¹.

Relationship	Expected		Observed	Total no. of pairs
	(Autosomal)	(Sex-linked)		
Father-son	0.50	0.00	0.25	188
Mother-daughter	0.50	0.50	0.42	207
Mother-son	0.50	0.71	0.38	188
Father-daughter	0.50	0.71	0.40	207

Comparison with the expected values of the correlations for autosomal and sex-linked inheritance of the intermediate type show that the results are suggestive of the operation of sex-linked factors, without, however, being at all conclusive. The mother-child correlations are somewhat higher than the corresponding father-child correlations and this may be due either to the more intimate association of young children with the mother or to doubtful paternity.

Another way of testing for sex-linked factors is to measure the likeness between brothers and sisters. With sex-linked inheritance the likeness between sisters should be closer than that between brothers, and both these likenesses should be greater than the similarity between brothers and sisters. Taking, in turn, all the pairs of brothers, sisters, and brother and sister in the 100 families, correlations for intelligence having the values 0.18, 0.14 and 0.15 respectively are obtained. The low values of these coefficients are partly due to the fact that the fraternities are all selected as the result of the presence of at least one mentally defective

¹ Cf. Hogben (14).

member. There is no significant difference between their relative magnitudes. There is, however, some doubt as to how far the ordinary methods of correlation are applicable to these brothers and sisters. They are not picked out at random from a large sample, but whole fraternities are considered. Let us examine in detail the method of obtaining these coefficients.

If, in a given fraternity, x , y , z and w are the number of brothers in each of the four classes, superior, normal, dull and defective, and if x' , y' , z' , w' are the number of sisters in these classes, then the number of pairs of various types can be shown thus:

		Brothers			
		Superior	Normal	Dull	Mentally defective
Brothers	Superior	$x(x-1)$	xy	xz	xw
	Normal	yx	$y(y-1)$	yz	yw
	Dull	zx	zy	$z(z-1)$	zw
	Mentally defective	wx	wy	wz	$w(w-1)$

Tables like this are made out for each family and when they are all added together the total number of pairs of brothers of each type is found. The table for sisters will be exactly similar, but that for brothers and sisters will be as follows:

		Sisters			
		Superior	Normal	Dull	Mentally defective
Brothers	Superior	xx'	xy'	xz'	xw'
	Normal	yx'	yy'	yz'	yw'
	Dull	zx'	zy'	zz'	zw'
	Mentally defective	wx'	wy'	wz'	ww'

Now, it is clear that if, for example, $x = x'$, that is to say, there are, in a family, an equal number of brothers and sisters of one type, the number of like pairs of brothers is less than the number of corresponding like pairs of brothers-sisters, for $x(x-1) < x^2$. Hence, we should expect the method to give values of the brother-brother and sister-sister correlations too low in comparison to the brother-sister correlation. To correct this discrepancy, every brother can be regarded as being the brother of himself, and, in view of the way the correlations for siblings are calculated theoretically, this seems correct. The table giving the number of pairs of brothers (or sisters) of each type then becomes:

		Brothers			
		Superior	Normal	Dull	Mentally defective
Brothers	Superior	x^2	xy	xz	xw
	Normal	yx	y^2	yz	yw
	Dull	zx	zy	z^2	zw
	Mentally defective	wx	wy	wz	w^2

which is strictly comparable with the brother-sister table. The families can also be weighted, by dividing each table by its total sum, so as to make them all of unit size, but this manipulation has very little effect on the final values of the correlations and neglects the fact that large families are much more important than small ones.

The effect of counting each person as a like-brother (or sister) of himself in the data considered here is to raise the brother-brother and sister-sister correlations, as shown in Table VII. Only sibships of at least two known members were included in the data from which these revised correlations were derived.

Table VII. *Fraternal correlations (heterozygote intermediate).*

Relationship	Expected		Observed
	(Autosomal)	(Sex-linked)	
Brother-sister	0.50	0.35	0.15
Brother-brother	0.50	0.50	0.44
Sister-sister	0.50	0.75	0.37

These figures are quite suggestive of the operation of sex-linked factors, but objections which might be made to the method of obtaining them have to be taken into account before any inferences can be drawn from them.

On general grounds it cannot be considered that sex-linked factors are very likely to be found in examining the genetics of the subcultural group of mental defectives, since the chances are that they would only form one twenty-fourth of the multiple hereditary factors which may be responsible. Against this argument, it may be pointed out that there are suggestions of the existence of differential assortative mating of parents with respect to mentality, for defective males and normal females do not seem to mate. A circumstance of this kind persisting through a long series of generations would tend to accentuate the effects of sex-linked factors.

VII. CONSANGUINITY.

Goddard considered mental defect to be inherited as a single Mendelian recessive trait. When a recessive trait is very rare an extremely useful test is given by the proportion of parents having affected offspring who are consanguineous. According to Lenz⁽¹⁵⁾ and others, if a is the incidence of first cousin marriages in the general population and p^2 the incidence of the recessively determined character, the incidence of first cousin marriages among parents with affected offspring is $\frac{a}{a + 16p}$. In

the present instance, 5 per cent. of the parents are first cousins and we may take 0.8 per cent. as the normal incidence of first cousin marriages. If the trait we are examining only presented itself in one in every 10,000 of the general population, 5 per cent. of consanguineous parents would be the expected value. But subcultural mental deficiency is very common: its incidence is probably at least $1/400$. The incidence of mental deficiency in the general population is nearly 1 per cent., and the subcultural group form something like one-quarter of all mental defectives. Therefore a significant excess of consanguineous parents would not be expected, were it determined by a single recessive gene. We must look elsewhere for the explanation of the high proportion of first cousin marriages among these parents¹. Either we have struck some rare recessive trait which is acting as one of the alternative hereditary factors in certain cases, or else here is just another example of the tendency of like to mate with like in these families.

VIII. CONCLUDING REMARKS.

The study which has been described in this paper forms part of the research scheme which is in progress at the Royal Eastern Counties' Institution under the auspices of the Medical Research Council and the Darwin Trust. A very large amount of the family investigation, on which these results are based, was carried out by Miss D. Newlyn, and my thanks are due to her for careful and accurate work. As previously stated, this study is only a preliminary piece of research. The conclusions may be summarized as follows.

The relationship of intelligence of children to that of their parents and to one another has been studied in 100 families containing subcultural mental defectives. The likenesses found can be roughly accounted for by the assumption of alternative additive Mendelian genetic factors, a noticeable proportion of which are sex-linked.

Although cases have been excluded from the survey in which there was any suggestion of secondary causes of mental deficiency, environmental factors may yet have been present which tended to alter the intelligence of the persons studied. Bad home conditions are connected with low intelligence of parents, but there is little evidence that the bad conditions by themselves cause low mentality in the children. On the other hand, the effect of age and parity of the mother on the intelligence of the child seems likely to be of importance.

¹ A similar effect was noticed by Sjögren(4).

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(Manuscript received, 17 October, 1932.)

APPENDIX.

In the following list of 100 families, *N* denotes *normal* intelligence, *D* denotes *dull* and *M* denotes *mentally defective*, + indicates *superior intelligence*, an asterisk signifies that the parents are first cousins.

Out of the 597 children in these families, 202 individuals were of unknown mental capacity; 61 of these unknowns were miscarriages or stillbirths, 66 died in infancy, 10 were alive but too young to test, 52 were adults alive but not traced and 13 were adults who had died.

		Father <i>N</i> Mother <i>N</i>	<i>N</i> <i>D</i>	<i>N</i> <i>M</i>				
Sons	+	2	.	.				
	<i>N</i>	58	7	5				
	<i>D</i>	9	8	6				
	<i>M</i>	25	5	11				
Daughters	+	5	.	.				
	<i>N</i>	61	10	6				
	<i>D</i>	4	5	8				
	<i>M</i>	24	8	9				
Unknown		85	10	17				
		Father <i>D</i> Mother <i>N</i>	<i>D</i> <i>D</i>	<i>D</i> <i>M</i>				
Sons	<i>N</i>	8	7	.				
	<i>D</i>	1	4	1				
	<i>M</i>	4	8	11				
Daughters	<i>N</i>	11	1	.				
	<i>D</i>	6	8	.				
	<i>M</i>	7	9	7				
Unknown		23	36	13				
		Father <i>M</i> Mother <i>N</i>	<i>M</i> <i>D</i>	<i>M</i> <i>M</i>				
Sons	<i>N</i>	.	.	1				
	<i>D</i>	.	.	1				
	<i>M</i>	.	.	6				
Daughters	<i>N</i>	.	.	.				
	<i>D</i>	.	.	2				
	<i>M</i>	.	4	12				
Unknown		.	3	15				
		Father <i>N</i> Mother <i>D</i>	10 families.					
Serial number	Sons			Daughters			Un- known	Total
	<i>N</i>	<i>D</i>	<i>M</i>	<i>N</i>	<i>D</i>	<i>M</i>		
39	1	.	1	1	.	.	.	3
56	1	.	1	.	.	.	1	3
*91	1	1	.	.	1	1	1	5
109	.	.	.	1	1	1	1	4
186	.	.	1	.	.	.	1	2
203	2	2	.	2	.	3	3	12
237	.	.	.	2	.	1	.	3
335	.	.	1	.	.	.	.	1
503	.	2	1	1	.	.	2	6
396	2	3	.	3	3	2	1	14

Serial number	Sons				Daughters				Un- known	Total
	+	N	D	M	+	N	D	M		
*37	.	3	.	1	.	.	.	.	2	6
59	.	1	.	1	1	2	.	.	.	5
97	.	.	.	1	.	1	.	.	.	2
106	.	.	.	1	.	.	.	.	.	1
105	.	1	1	1	.	1	.	.	2	6
108	.	1	.	1	.	.	.	.	1	3
150	.	1	.	1	.	.	.	.	.	2
161	.	1	.	1	.	2	.	.	1	5
163	.	3	.	1	.	3	.	.	3	10
167	.	.	.	1	.	1	.	.	1	3
239	.	3	.	.	.	2	.	1	.	6
253	.	4	.	.	.	4	.	1	4	13
279	.	5	.	.	.	3	.	1	7	16
285	.	.	.	.	.	.	.	1	.	1
327	.	3	.	.	.	7	.	1	1	12
352	.	4	.	.	2	3	.	1	3	13
354	.	1	2	1	.	3	.	.	7	14
362	.	1	.	.	.	3	.	1	5	10
379	.	1	.	.	.	1	.	1	.	3
249	.	1	.	.	.	1	.	1	.	3
254	.	.	.	.	.	1	.	1	.	2
260	.	3	.	1	.	1	.	.	1	6
290	.	2	.	.	.	.	.	1	5	8
328	.	1	1	.	.	1	.	1	4	8
356	.	3	1	.	.	3	.	1	4	12
400	.	3	.	.	.	2	.	1	1	7
413	.	1	.	.	.	1	.	1	1	4
418	.	1	.	.	.	2	1	1	3	8
434	.	.	.	.	.	.	.	1	.	1
467	.	2	.	.	1	2	.	1	3	9
478	.	.	.	1	.	.	.	.	.	1
480	.	.	.	1	1	.	.	1	1	4
485	.	1	.	1	.	.	.	.	1	3
505	.	.	.	1	.	1	.	.	.	2
506	.	1	.	1	.	.	1	.	7	10
510	.	.	.	1	.	.	.	.	.	1
519	.	1	.	2	.	2	.	.	1	6
531	1	1	.	.	.	1	1	1	.	5
596	.	1	.	.	.	2	.	1	6	10
265	.	.	.	1	.	.	.	1	.	2
641	.	.	.	1	.	.	1	.	1	3
*364	.	1	.	.	.	3	.	2	1	7
459	1	1	2	1	.	.	.	.	5	10
499	.	.	1	1	.	1	.	.	2	5
384	.	1	1	1	.	1	.	.	1	5

	Father D } 8 families. Mother N }							
	Sons			Daughters				
Serial number	N	D	M	N	D	M	Un- known	Total
126	2	.	.	3	.	1	5	11
*168	.	.	1	2	1	1	.	5
353	1	.	.	.	2	2	5	10
397	1	.	.	3	.	1	1	6
227	1	1	1	1	.	.	6	10
311	1	.	.	.	2	1	4	8
608	2	.	2	2	1	.	.	7
391	.	.	.	.	.	1	2	3

A Study in the Inheritance of Intelligence

Father *N* } 12 families.
 Mother *M* }

Serial number	Sons			Daughters			Un- known	Total
	<i>N</i>	<i>D</i>	<i>M</i>	<i>N</i>	<i>D</i>	<i>M</i>		
28	1	1	1	1	1	.	3	8
107	.	.	1	.	.	.	.	1
200	1	.	1	1	1	1	1	6
240	.	1	1	.	2	1	5	10
563	.	.	2	1	.	.	1	4
264	1	.	.	.	.	3	3	7
204	.	.	.	.	.	1	.	1
419	.	1	1	1	.	.	1	4
523	.	1	1	1	.	.	1	4
714	.	2	1	1	1	.	.	5
513	2	.	2	.	3	2	2	11
365	.	.	.	.	.	1	.	1

Father *D* } 9 families.
 Mother *D* }

Serial number	Sons			Daughters			Un- known	Total
	<i>N</i>	<i>D</i>	<i>M</i>	<i>N</i>	<i>D</i>	<i>M</i>		
*46	.	1	3	.	3	1	5	13
96	.	.	1	.	.	.	.	1
179	2	.	1	.	1	1	.	5
225	.	1	.	.	.	1	1	3
322	3	1	1	.	2	1	9	17
548	.	.	.	.	.	3	1	4
430	2	.	.	.	.	1	11	14
628	.	1	1	1	2	.	8	13
314	.	.	1	.	.	1	1	3

Father *D* } 7 families.
 Mother *M* }

Serial number	Sons			Daughters			Un- known	Total
	<i>N</i>	<i>D</i>	<i>M</i>	<i>N</i>	<i>D</i>	<i>M</i>		
460	.	.	1	.	.	.	.	1
644	.	.	5	.	.	2	2	9
399	.	.	1	.	.	3	4	8
80	.	.	1	.	.	.	.	1
100	.	1	2	.	.	.	2	5
303	.	.	1	.	.	1	2	4
321	.	.	.	.	.	1	3	4

Father *M* } No families.
 Mother *N* }

Father *M* } 2 families.
 Mother *D* }

Serial number	Sons			Daughters			Un- known	Total
	<i>N</i>	<i>D</i>	<i>M</i>	<i>N</i>	<i>D</i>	<i>M</i>		
438	.	.	.	.	.	3	3	6
555	.	.	.	.	.	1	.	1

Father M } 7 families.
 Mother M }

Serial number	Sons			Daughters			Un- known	Total
	N	D	M	N	D	M		
134	.	1	.	.	1	2	.	4
104	.	.	2	.	.	2	2	6
281	.	.	1	.	.	2	.	3
289	.	.	1	.	1	2	8	12
450	1	.	1	.	.	1	2	5
715	.	.	.	.	.	3	3	6
716	.	.	1	.	.	.	.	1

TEMPERAMENT TESTS. II. TESTS.

By RAYMOND B. CATTELL.

- I. *Clues to the construction of temperament tests* (pp. 20-1).
- II. *On the nature of perseveration and the tests devised to measure it* (pp. 22-6).
- III. *Intercorrelations of old and new perseveration tests* (pp. 26-9).
- IV. *The relation of perseveration to character and temperament* (pp. 29-38).
- V. *Tests of speed, oscillation, intelligence, physique and the psychogalvanic reflex related to the 'c', 'w', 'a' and 'm' temperament and character factors* (pp. 38-45).
- VI. *Some problems for further research* (pp. 45-7).
- VII. *Summary* (pp. 47-8).
- References* (p. 49).

I. CLUES TO THE CONSTRUCTION OF TEMPERAMENT TESTS.

IN a previous paper (5) I have examined evidence which shows that, to a first analysis of character and temperament qualities, two broad patterns or types are revealed. These depend on a general character factor 'w' and a general temperament factor 'c.' The type possessing a high degree of 'c' I have called the Surgent temperament and the opposite type the Desurgent temperament, since no temperament types yet named seem to fit adequately the patterns established through these experimental enquiries. The surgent temperament is characterized by high cheerfulness, sociability, quickness of apprehension, impulsiveness and originality: the desurgent by the opposite qualities. There is less definite evidence pointing to the existence of two or three additional, independent and smaller group factors, one of which has been designated by 'm' for 'maturity' and the other by 'a' for 'adjustment'. The latter is probably the basis of the cyclothyme-schizothyme type distinction.

The present paper describes an attempt to discover brief laboratory tests which will correlate with these broad behaviour patterns thus established. In the search for tests guidance was obtained from the study of previous experimental work, from the trend of psychological discussion, from the findings of abnormal psychology and from close personal observation of the manner of thought and reaction in people belonging to well-marked temperament types.

Speculation has long connected high perseveration with introversion and sometimes with steadfastness of purpose. Recent experimental work, on the other hand, shows no connection to exist with well-known temperament types, and an inverse relation to hold with steadfastness and goodness of character. One aim of the present research was to bring further evidence to bear on this connection of perseveration with character.

A direction in which to look for tests of the surgent temperament is suggested by the unequivocal evidence of Garnett⁽⁶⁾, Hargreaves⁽⁷⁾ and Bernstein⁽¹⁾ to the effect that surgency—the 'c' factor—is particularly closely connected with Quickness of Apprehension, Imagination, Capacity for Rapid Mental Work and a high score in actual speed tests. Measurement of the subject's speed of reading and quickness of decision was therefore a part of our examination. A characteristic of people of surgent temperament which had impressed the present writer was the spontaneity and vitality of their conversation, the ease and fluency with which association of ideas took place (not so often by eduction as by reproduction), and the 'volume' of their cognitive output in a given time. For this reason the 'cognitive output' test was devised; a test to measure the volume of association of ideas when logical aids are as far as possible eliminated. Next, a test of oscillation (reversible perspective) was included, because McDougall and Smith⁽¹⁰⁾, in a preliminary research with a few subjects, found indications that it might be a very valuable test of 'introversion-extraversion.' Spearman⁽¹⁵⁾ has also suggested that oscillation may be associated with important temperament traits.

There have been many attempts to employ the psychogalvanometer in the diagnosis of various normal and abnormal types and these have met with some measure of success. Elsewhere⁽⁴⁾ I have summarized evidence which shows that the psychogalvanic reflex has low positive correlations with cheerfulness, vigour, 'formidableness of personality,' quickness of decision and with conative qualities generally. From this it is evident that one might expect the psychogalvanic reflex to give a good measure of surgency, and it was included in our apparatus. Finally, an intelligence test was added to confirm, or otherwise, the independence of 'c' and 'w' with respect to intelligence and to determine what part intelligence might play in other character factors. All these tests are described in more detail in the appropriate parts of the succeeding sections. The subjects of the tests were the 62 training college students upon whom the trait estimates described in the previous paper had been made.

II. ON THE NATURE OF PERSEVERATION AND THE TESTS DEVISED TO MEASURE IT.

When devising perseveration tests and discussing appearances of perseveration one must not overlook the possibility that the variety of perseverative processes which are recognizable in the normal and abnormal mind may be of fundamentally different origins and natures. *A priori* one might consider the following variety of origins for processes to which the term perseveration could be (and indeed has been) applied. (1) Perseveration of response through the referring of different ideas and stimuli to a single major sentiment or complex (delusions, melancholia, consistency of character). (2) Perseveration due simply to mental asthenia and lack of spontaneity, which permits any process of thought or action once started (by external stimulation) to proceed unusually long without interruption. (3) Perseveration due to the very nature of the nervous tissue and analogous to the inertia of the physicist, *i.e.* something which shows itself as a lag in all nervous processes, resulting in some interference of all consecutive mental activities. (4) Perseveration as an obstinacy of old habits in the face of habits being newly formed, or *vice versa*. (5) Quite apart from these varieties of what might be called true perseveration one meets instances, such as the mental defective's useless repetition of old and inappropriate responses in new situations, *e.g.* Binet test situations, which are really due to quite unrelated causes, usually low 'g.' In the latter case perseveration is due to an urge to act which is not guided by any appreciation of the form of the new response required.

One may ask further whether perseveration applies equally to all kinds of experience—conative, affective and cognitive. If perseveration is of the nature of an inertia, will the subject who shows marked sensory inertia in, say, the Masson's Disc experiment (Heymans and Wiersma) also show perseveration of emotional moods and conative sets? By choosing tests of a suitably varied nature it should be possible to answer many of these questions. In those researches which have established the existence of a general factor of perseveration the tests have been, with very few exceptions, of a motor type. Whatever new tests we might take, therefore, we had to employ at least a nucleus of old and well-tried tests of this motor or sensory type. For, firstly, we had to be sure that we were dealing with the same general perseveration factor as has been the subject of discussion in previous work and, secondly, in the event of the newly devised tests proving worthless as tests of perseveration, we were obliged to have some reliable measure of perseveration to which to relate the

temperament estimates. Limited by these necessities, our tests could not cover the whole field of possible perseveration processes discussed above. From the results of research which were available at the time the present work was begun (principally those of Lankes (9), Wynne Jones (20), Bernstein (1), Pinard (11) and Stephenson (17)) we concluded that the most reliable and valid tests would be found in the *triangles test*, the *cancellation test* and the *reversed letters test*. To this nucleus were added tests of conative, cognitive and affective perseveration, a measure of rhythm and also two tests of 'persistence.' The latter term refers to a phenomena studied by Cathcart and Dawson (3) which appeared to the present writer to be almost certainly related to perseveration and worth investigating from this standpoint.

Briefly described, the final battery of tests was as follows:

(1) *Triangles*. Instruction to draw triangles thus, $\triangle\triangle$, as rapidly as possible. Mark affixed at end of half minute. Continued for another minute. Changed without pause to drawing triangles thus, $\nabla\nabla$, for a further minute.

$$\text{Perseveration score} = \frac{\text{Number of triangles in last minute (X)}}{\text{Number of triangles in preceding minute (Y)}}$$

Repeated after long interval, starting with triangles thus, $\nabla\nabla$, and changing to thus, $\triangle\triangle$.

(2) *Reversed letters*. Arrangements precisely as for triangles, substituting 'opqrs' and 'tsrqpo' for $\triangle$ and ∇ .

(3) *Cancellation*. As for triangles, cancelling T and X only (on prepared sheet presenting rows of mixed letters) in place of $\triangle$, and O and I in place of ∇ .

(4) *Forearm rhythm*. Subject, who sits comfortably in chair, instructed to beat out with the forearm (elbow resting on chair arm and forearm stiff), a steady time at a rate which seemed most normal, natural and comfortable to himself. Amplitude limited to 6 in. by mechanical obstruction. First five beats neglected, then timed for half a minute.

(5) *Persistence of rhythm*. Five seconds after completion of above forearm rhythm test a metronome was started at *half* the speed adopted by the subject. Subject synchronized with metronome for 1 min., then, after 10 sec. interval, was instructed to regain his natural rate; timed for 20 sec.

$$\text{Score} = \frac{\text{Original speed} - \text{final speed}}{\text{Original speed} - \text{imposed speed}}$$

i.e. a measure of the deflection from the original, natural speed produced by perseveration of the imposed speed.

(6) *Persistence of colour image*. This is the second of the two tests adapted from Cathcart and Dawson (3).

- (1) Subject shown pale red and asked to remember shade.
- (2) Shown series of shades 1-10 and asked to pick out (1) from memory.
- (3) Shade at (1) again shown.
- (4) Much deeper shade shown, with instruction "Fix it in mind."

(5) Series again shown, with instruction "Pick out (1) as before."
Test repeated with another shade of red and two shades of blue.

Persistence (of effect of (4)) = Difference of shade chosen in (5) and (2). Difference of (1) and (2) taken as a control measure of accuracy of visual memory.

(7) and (8). *Rhyming and association*. List of words exposed (*hat, pod, eyes*, etc.) with instructions to write down a rhyme to each as quickly as possible. Immediately afterwards a second list exposed with instruction to write down *very first word of association*. Second list designed to be (a) provocative of rhymes and (b) of associations taken from first list, so that a measure might be obtained of the tendency for perseveration of a conative set and of cognitive associations.

Rhyming perseveration = Number of rhymes given in second list.

Association perseveration = Number of association words carried on either from (a) first list, or (b) subject's responses to first list, or (c) earlier part of same list¹. Repeated same evening and later.

(9) *Feeling perseveration*. Single words exposed in quick succession, with instructions "Write down at once *P* or *U* according as impression is pleasant or unpleasant." Words in groups of six, first word always markedly pleasant or unpleasant; following five as neutral as possible¹. Six sets of six words exposed and similar set later.

Perseveration score = Number of *P* reactions after initial *P* words and *U* reactions after initial *U* words.

(10) *Apperceptual perseveration*. A number of words were exposed singly, each for a fraction of a second. Immediately before their exposure the subjects were given a talk on a single theme (in the first experiment athletics, in the second classroom teaching) from which all the words in the ensuing list were taken. Most of the words had one or more letters misspelt and the subjects were told, "Some of the words may be misspelt: write them down exactly as you see them." Subjects having a marked perseveration of the ideas and 'apperception masses' previously stimulated would presumably be more likely to fill in the unclear and wrong spelling of the exposed words with the correct supplementation, i.e. to overlook words wrongly spelt. Each subject's score was made with reference to his 'perceptual accuracy' determined by exposing a similar list of mixed words unrelated to any recent subject of conversation. Score for apperceptual perseveration = Errors overlooked on the recently discussed topic list - Errors overlooked in list of general and indifferent topics.

(11) *Attitudes perseveration*. The lag and interference in two kinds of attitude change was tested: (1) with respect to a change of attitude from flippant to serious (and *vice versa*), (2) in a change of attitude from hostile-critical to friendly-confirmatory (and *vice versa*). Instruction "I shall read out ten questions. Write down quickly a flippant answer to each. Immediately afterwards, as the same ten questions are read out again, write down a serious and reasonable answer to each, e.g. 'What is a mariner's compass?' 'About ten gallons!' and later 'A device for keeping the compass card level in spite of the ship's movements.'" The reverse change was studied with another ten questions and the other attitude change with a series of ten statements, to which a critical rejoinder had to be made in the one case and a helpful, confirmatory remark in

¹ The exact details of administration and scoring of this and other tests will be found in a thesis in the University of London Library entitled *Temperament Tests and Perseveration*.

the other. The scoring threatened to be somewhat arbitrary and subjective, but was made by two examiners working independently, who endeavoured to keep an unvarying standard.

Perseveration score

$$\frac{\text{Number of correct (suitable) responses to first ten questions or remarks} - \text{Number of correct (suitable) responses to repetition of ten questions or remarks}}{2}$$

(Mean of both directions of change and both kinds of change.)

Throughout the above tests runs the general principle of measuring the subject on a first activity (which we may call the X activity) and, immediately afterwards, on a second, interfering activity (which we will call the Y activity). This is typical of most perseveration tests now in use and the perseveration effect is measured by the degree of interference found in the Y activity; but as to the exact method of evaluating it there is much disagreement. Lankes⁽⁹⁾, Wynn Jones⁽²⁰⁾ and Hargreaves⁽⁷⁾ calculated perseveration as X/Y , i.e. as speed of first activity over speed of hindered activity. Bernstein⁽¹⁾, Pinard⁽¹¹⁾ and Jasper⁽⁸⁾, on the other hand, took as a perseveration index simply the difference of the two measures, i.e. $X - Y$. The latter method must be considered unsatisfactory because it includes within itself a measure of speed, which presumably is quite different from perseveration. For the value $X - Y$ will be large, quite apart from any increase of perseveration, if X and Y are large, i.e. if the subject is naturally a rapid writer. Quite large spurious correlations among perseveration tests could clearly result from employing such a formula. Not having foreseen this possibility we began to work out the results of the present research on the $X - Y$ formula and had to rescore the tests at an advanced stage on the X/Y formula. The inter-correlations thus far calculated fell somewhat on being recalculated according to the new formula, but the perseveration score then ceased to have any significant correlation with speed alone. Since the completion of this work Rangachar⁽¹³⁾ has introduced a more thorough but more laborious technique for eliminating speed, in which the $X - Y$ formula is first used and then X is multiplied by a constant calculated from Spearman's formula for correlations of sums and differences to give no correlation between speed and perseveration. It is, of course, possible that there should be some correlation between perseveration and speed in certain circumstances.

A new arrangement of the test itself which has some promise consists in getting a measure of the X activity, then (without promoting interference) of the Y activity, and finally demanding a rapid alternation of the X and Y activities. For example, in the S's test, the subject writes

SSSSSSSS for half a minute, then, after a pause, 22222222 for half a minute, and finally S2S2S2S2 for 1 minute. The total for the first two sets is then compared with the total for the last set according to the most satisfactory *X* and *Y* formula. This arrangement, which has been called by Babington Smith the alternating test, as distinct from the original hindrance test, has several obvious advantages. But in most of the examples so far devised it has also the disadvantage of measuring to some extent '*g*,' for the more intelligent subjects are quicker to see S2 as a single configuration and to write it as such. Stephenson has shown that there is practically complete correlation between the hindrance and the alternating form of any particular test, so they do at least measure the same thing, but there remains considerable need of investigation into the detailed technique of perseveration tests, especially if they are to take a place in clinical practice.

III. INTERCORRELATIONS OF OLD AND NEW PERSEVERATION TESTS.

From the very beginning the intercorrelations of the perseveration tests turned out low and insignificant, even for a field in which low intercorrelations are the rule. These low and occasionally negative correlations ceased to be so surprising, however, when it was found that the reliability coefficients of the tests were, with two exceptions, negative! Now it will be remembered that the second part, or repetition, of each test was given with the two activities in the reverse order to that found in the first part. For instance, in the letters test, on the first testing the *X* activity consisted in writing *opqrst* and the *Y* activity in writing *tsrqpo*. On the second testing, which succeeded the first by a few minutes, the *X* activity was writing *tsrqpo* and the *Y* activity *opqrst*. The perseveration score was then worked out as $\frac{X_1}{Y_1} + \frac{X_2}{Y_2}$, i.e. on the notion that perseveration shows itself

as an inertia in changing from one activity to another, independently of the nature of the activity. This reversal of the parts of the test was an attempt to eliminate the effect of any facility in writing *tsrqpo* as against *opqrst* and to leave only the effect of changing over from one activity to an interfering (because inverted) activity. Our intercorrelations were so insignificant as to give scarcely any ground for supposing the existence of an effect of this kind, i.e. of an 'inertia perseveration' independent of whether the change is from old to new or new to old habits. We therefore rescored the tests once more, reversing the sign of the second portion, so that, in the letters taken as an example above, perseveration was taken

as the speed of writing *opqrst* divided by the speed of writing *tsrqpo*, without regard to whether one preceded or followed the other.

The intercorrelations of the tests marked in this way were better than on the original conception of perseveration but were still very disappointing. If such findings are confirmed by further investigation of perseveration test techniques, a new conception of perseveration must be accepted, namely, perseveration as the readiness of functioning of an old-established habit relative to a new habit involving a rearrangement of the old elements. Perseveration as a general inertia, manifesting itself as a hindrance effect in passing rapidly from one activity to any other kind of activity, may indeed be the same thing, but it is in the disparity of fluency of old and new motor habits that the tendency is most readily revealed and by which we can best hope to measure it.

Table I. *Intercorrelations of revised tests.*

Test	A	B	C	D	E	F	G	H
A. Natural (fore-arm) rhythm	0.81	(-) 0.01	(-) 0.11	(+) -0.09	(-) 0.14	(+) -0.23	(+) -0.20	(-) 0.12
B. Conative (rhyming) ^{set} (-)	0.01	0.28	-0.02	0.02	0.01	-0.18	-0.03	-0.07
C. Triangles (-)	0.11	-0.02	0.41	0.22	0.08	0.20	-0.25	0.23
D. Letters (+)	-0.09	0.02	0.22	0.52	-0.09	0.06	0.02	0.03
E. Cancellation (-)	0.14	0.01	0.08	-0.09	0.26	-0.16	0.01	0.04
F. Persistence (rhythm) (+)	-0.23	-0.18	0.20	0.06	-0.16	0.75	-0.06	-0.01
G. Apperceptual perseveration (+)	-0.20	-0.03	-0.25	0.02	0.01	-0.06	-0.04	-0.08
H. Attitudes (-)	0.12	-0.07	0.23	0.03	0.04	-0.01	-0.08	0.34

The tests for colour persistence, perseveration of feeling tone and of associations have already been omitted, for in the first table their correlations were so insignificant as not to justify further work on them.

Many of the correlations of forearm rhythm are negative because the test is scored as rapid rhythm = high score, and here, as in previous researches, rapid rhythm is correlated with low perseveration. The true signs to be considered if it is accepted as a perseveration test are the reversed signs in brackets.

In spite of the low and negative correlations these coefficients were examined by the tetrad difference criterion for the presence of group factors. The tetrad differences arranged themselves in a normal distribution curve the quartiles of which were within the theoretical P.E. The highest tetrads were found in connection with the apperception-triangles relationship, the forearm rhythm—rhythm persistence relationship and

the correlations of apperception with some other tests. The nature of the overlap in the two latter tests, namely, the particular rhythm activity involved in both of them, is easy to appreciate; but no basis could be discovered for the other overlap. In any case there is no evidence of definite overlap, but only a slight suspicion of some connection. However, with the object of pooling the scores in the best tests only, it was decided to reject from the battery the apperception test (its reliability being so low in any case) and the cancellation test, on account of low correlations.

Of the remaining tests the highest mean correlation is found in the natural rhythm, attitudes, triangles and letters tests, but in none of them can the coefficients really be considered significant. The decision to make a pooled perseveration score from these tests, and to weight certain tests more than others, was accordingly based less on any evidence brought forward in the present research than on the experience of other research workers, notably Pinard (12) and Stephenson (16), to the effect that certain tests may be considered valid and that even with low intercorrelations a pooled score is of some value. Since the perseveration tests are only a minor part of this research we could not digress into special investigations of the causes of these low correlations. Pinard (11) has maintained that valid perseveration measurements can only be obtained by giving the subjects practice and throwing away the practice results. Unfortunately such a procedure would not allow the tests any future in clinical work, and, since there seems to be nothing in the intrinsic nature of perseveration requiring such conditions (indeed one would expect the best measurements to be obtained when the test is entirely unfamiliar), it seems best to continue experimenting with tests applied once only, in the hope of getting increasingly valid tests of this type.

It is of considerable theoretical interest that the attitudes test survives among the four best tests of our battery (as far as these correlations are indicative); for hitherto most tests in use have been of a motor and sensory type and the question has arisen (indeed it was considered in planning these tests) as to whether motor perseveration can be considered to be one with conative and affective perseveration. The present finding suggests that it can, and the relatively negative findings with our other tests of conative and affective perseveration are not to be taken too seriously; for the correlations were positive, if insignificant, and might be larger on the revised scoring, whilst the technique was by no means as satisfactorily worked out as in the older tests and might be much improved upon in the light of experience.

These results are of interest as indications of the nature of persevera-

tion and of the direction in which we may look to develop new tests, but for the purpose of getting the most reliable measurement of perseveration to which to relate character traits we must take a more conservative attitude and give relatively great weight to tests of established worth. The consensus of evidence is to the effect that letters is a good test, triangles is moderately good and cancellation is practically worthless. We compromised with our own finding, therefore, by computing each person's perseveration score as the pool of the scores on letters, triangles, natural rhythm, attitudes and rhythm persistence tests, the first two tests each being weighted to double the value of any one of the others. The intercorrelations of these five tests among themselves are given in Table II.

Table II. *Intercorrelations of tests used to get perseveration score.*

	Letters	Triangles	Rhythm persist- ence	Attitudes	Natural (forearm) rhythm
Letters	0.52	0.22	0.06	0.03	0.09
Triangles	0.22	0.41	0.20	0.23	-0.11
Rhythm persistence	0.06	0.20	0.75	-0.01	0.23
Attitudes	0.03	0.23	-0.01	0.34	-0.12
Natural (forearm) rhythm	0.09	-0.11	0.23	-0.12	0.75

IV. THE RELATION OF PERSEVERATION TO CHARACTER AND TEMPERAMENT.

When the pooled perseveration scores had been worked out in the above fashion, the correlations of perseveration with the surgent temperament factor ('c'), the general character factor ('w') and the smaller group factors ('a' and 'm'), established on the basis of observation of behaviour in the previous paper, could be investigated. The resulting correlations are given in Table III.

Table III. *Perseveration related to character and temperament.*

	Surgency 'c'	Character 'w'	'm'	'a'
Perseveration	0.11	-0.21	0.02	0.05
P.E.	0.08	0.08	0.09	0.09

Of these the only one approaching significance is the negative relation to the general character factor. This is in agreement with the previous finding of Lankes(9), who obtained a low negative correlation of perseveration with 'persistence of motive,' or 'w,' contrary to general expectation. When corrected for attenuation this correlation seems to be

about -0.4 . A surprising discovery is the absence of any significant connection of perseverance with the surgent temperament; for a considerable body of speculative writing, which has in some places passed into accepted belief, identifies high perseverance with the desurgent ('introvert') temperament. Indeed, since Lankes showed that perseverance could not be regarded as the basis of steadfastness of purpose, there has been an especial proneness to believe that it is to be identified with 'introversions' or the 'schizothyme' temperament. In the previous part of this contribution we have shown that there is some reason for regarding the 'a' factor as the core of the cyclothyme-schizothyme distinction. If this should be confirmed our results would indicate that the connection of cyclothyme or syntonio type with low perseverance, asserted to exist by Kretschmer and others, has no experimental support, at least within the limits of normality.

Are we to regard the low negative correlation of 'p' with the general character factor as resulting from the reduction, through the accidental incidence of favourable and unfavourable environmental influences, of a much more perfect correlation of 'p' with constitutional tendency to good character integration? Here we must consider Pinard's evidence⁽¹²⁾ to the effect that in the group of children which he studied the most obviously difficult and unstable were found either among the very high or the very low perseverators. The subjects of our research were students selected for high moral standing by the bodies awarding places in the college, and there were no obvious or grave delinquents among them. They were radically different from Pinard's group also in average age, intelligence quotient, education and home background. It is therefore of considerable interest to discover whether the same relationship can hold in this group; whether those of bad character, or shall we say poor character (relatively low 'w'), are to be found principally amongst the very high or very low perseverators. To get groups separated by equal increments of 'p' the subjects were divided into seven groups according to the binomial distribution curve, as used in the previous estimates when it was desired to derive equal increments of deviation from the ranking order of the subjects. That is to say, after ranking the subjects for perseverance they were parcelled out into the following successive groups: 1, 6, 14, 20, 14, 6, 1, this being the best normal distribution for 62 subjects. The mean 'w' score for each group was then worked out, as shown in Table IV.

The relationship observed by Pinard clearly holds equally well for this very different group of subjects. Except for the average of the fifth

sub-group the '*w*' averages fit into a curve of the form shown in Fig. 1, suggesting lower '*w*' among those of high '*p*' than those of low '*p*'.

Table IV. *Relation of 'w' to increasing perseverance.*

No. of persons in group	1	6	14	20	14	6	1
' <i>p</i> ' score in deviation units	-3	-2	-1	0	1	2	3
' <i>w</i> ' score, mean for group	-3.0	-0.16	1.4	1.3	-1.17	-0.16	-10.0

Unless there is reason to believe that our group is in any way selected for perseverance—for high perseverance and therefore lacking the lower ranges—we must suppose that the curve for people in general has this asymmetrical form, finishing lower on the high perseverance than on the

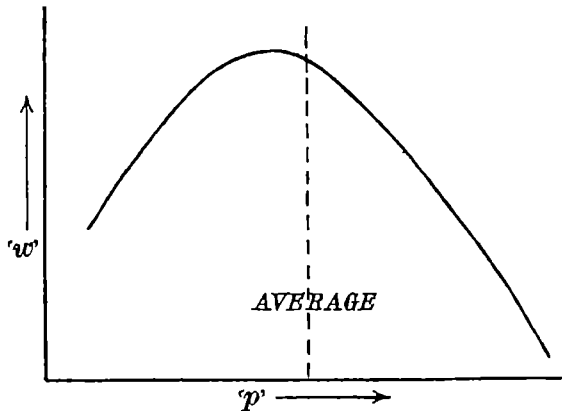


Fig. 1. Relation of Perseveration to Character.

low perseverance side. And this form agrees with the low negative correlations found between '*p*' and '*w*.' Presumably, therefore, a much higher correlation of '*p*' and '*w*' would be found if the relationship were worked out, in accordance with the non-linear connection revealed here, as a non-linear correlation coefficient. Thus the predictability of '*w*' from '*p*' is much higher than a correlation of -0.21 (or -0.4) would indicate.

Though both very high and very low perseverators have a low '*w*' value, it is reasonable to suppose that there will be some difference between them in their manner of falling short of high '*w*,' i.e. of good character integration as expressed in persistence of motive, conscientiousness, reliability, steadiness of emotional mood, etc.

In searching for these differentiating characteristics we first correlated the '*p*' score with each of the 48 character and temperament traits estimated in the previous research. Scarcely one significant correlation was

found: the qualities behaved truly as parts of the larger group factors, 'c' and 'w,' which we had isolated from them, i.e. those associated with 'c' uniformly had no relation to 'p,' and those saturated with 'w' uniformly had low negative correlations. This may be regarded incidentally as additional support for the integral unity of these constellations of traits. Two of the correlations are worth setting down here because the traits concerned have frequently been thought to depend on perseveration.

	Conservatism (opposed to adaptability)	Obstinacy (opposed to amenability)
Perseveration	0.004 (P.E. 0.09)	-0.32 (P.E. 0.08)

Perhaps the large element of organized will, even in the most quixotic obstinacy, accounts for the latter correlating as negatively with 'p' as does 'w.'

Having failed to establish any marked difference between high and low perseverators on the basis of the trait estimates used in the general research we resorted to the less exact and more primitive method of contrasting the personalities of the six highest with the six lowest perseverators of the group, simply on the basis of observation. Evidently perseveration differences are responsible for differences in more subtle and involved personality traits than those broad and obvious traits estimated in the earlier part of this research. Our method must be regarded as one which lays no claim to establishing connections of traits with perseveration, but merely indicates traits the extent of the connection of which must be confirmed by more controlled methods. The following traits are those on which agreement was reached by the four student judges, the college tutor and the experimenter. Although for the sake of brevity it is usual to describe traits by single words, the practice is always liable to be misleading and in the present instance particularly it seems essential to describe the modes of behaviour more fully. For example the terms 'effeminate' and 'sensitive' have been applied to high perseverators, but different individuals interpret these terms very differently and indeed there are widely dissimilar modes of behaviour gathered under such labels. The following traits are arranged in descending order of reliability; those at the top of the list are more certain and more widely agreed upon.

EXTREME LOW PERSEVERATORS.

Vital, tense, capable of great industry and continuous work at high pressure. 'Tough skinned,' insensitive to effects on others' feelings; apparently inconsiderate or even selfish, aggressive.

EXTREME HIGH PERSEVERATORS.

Languid, slow or desultory workers. Quiet. Sensitive to disagreements with others, accommodating, thoughtful of others (not necessarily more altruistic), suggestible.

EXTREME LOW PERSEVERATORS.

Uncompromising individuality, emphatic and sometimes awkward uniqueness of personality. Sometimes regarded as a tendency to 'act' (use of gesture, epigram and cultivated idiosyncrasy) for effect and for bluff.

Brusque; direct and downright expression of opinion. Sincere in all fully conscious thought.

Practical. Useful with their hands in some skilled activity. Good at drawing and caricature.

More astute. Better critical judgment in art. Able to rise above a sea of detail and grasp trends (*e.g.* in conversation). Sincerely at one with their own feelings.

Satisfied with a small circle of friends.

Religion a matter of practical morality.

Not much concerned with other's business.

More integrated. Moved as a whole to definite action, which may not be well considered and is often directed by immediate expediency. Practical and quick in emergencies.

More brittle; severely shaken when feelings are moved by real crisis. Feelings definitely expressed and definitely set aside in due course. Feelings are a direct response to the situation.

Sharp division between conscious mind which is well adapted to reality and the unconscious mind which expresses itself antipathetically by 'slips,' prejudices and physical symptoms obvious to the observer.

EXTREME HIGH PERSEVERATORS.

'Correct' in a negative way. More easily inhibited. 'Gentlemanly' (from the college tutor).

Suave; seldom giving unmodified expression of opinion.

Dreamily imaginative.

More sentimental. More detached from and introspective of their own feelings. Easily lost in details and liable to get quite a distorted perspective.

Likes to keep many acquaintances and to feel a sense of community with a large group.

Morality only a part of mystical religion.

Suspicious, jealous and fault-finding. Very interested in other personalities.

Always aware of moods and the co-existence of conflicting feelings, vacillating, undependable. "Never consistently true to one roguery at a time" (fellow-students).

More plastic. Slow to realize full emotional import. Slow to recover. Outwardly unmoved; inwardly struggling towards slow emotional adaptation.

Less sharp division. Conscious mind less adapted to reality, more aware of its motives and engaged in continuous conflicts.

The last two or three items of these twelve distinctions are less certain than the earlier ones, partly because they contain a good deal of interpretation read into behaviour by the experimenter. They are nevertheless well considered and believed to represent important differences. Some of the above differences may be illustrated by the following incidents. On looking at the perseveration list as soon as it was compiled and after every student had been encountered during the visits for individual experiments, the experimenter found that he could conjure up a picture of every one of the six extreme non-perseverators but could recall nothing of the six highest perseverators (excepting one who had been interviewed

several times in connection with the organization of the experiments). The experimenter's assistant was likewise able to recall many more low than high perseverators. The high perseverators had indefinite, self-effacing, accommodating, and 'correct' personalities which had left no impression. Again, of the sixty-two students asked to participate in the experiments only two declined to fall into the scheme. Each of these baldly stated that he could see no personal advantage to be gained from taking the experiments and that he had plenty of other things to do with his time. Each discussed the matter frankly, with a complete disregard of the experimenter's feelings and of the wave of interest that was passing over the rest of the group. Each continued to sit for the experiments, without the slightest indication of any suppressed resentment, after having had certain tangible advantages pointed out to him. Both of these men were found in the six extreme low perseverators, one of them being the lowest of all. Although the low perseverators were more individualistic in themselves the high perseverators contained several people most extreme in their ideas—rebellious, cantankerous and original theorists, who, however, were quite ordinary and conventional in their personalities. Asked in a questionnaire (the full results of which we hope to publish later) to name (a) the people particularly liked, (b) the people particularly disliked (in the rest of the experimental group), the six non-perseverators together named 29 liked and 11 disliked, whereas the same number of high perseverators named 11 liked against 9 disliked, entering in each case into relative lengthy analyses of faults. This agrees with the reported hypercritical nature of high perseverators where persons are concerned.

These differences do not enable us definitely to establish a distinction between the ways in which high and low perseverators fall short of being considered sound characters, but taken in conjunction with the findings of Pinard and Stephenson they enable us to reach some general conception. Among children Pinard (12) found that high perseverators were rebellious, irritable, bad tempered and moody, while non-perseverators were 'petty,' nagging and whining, and happiest in a crowd. Among abnormal adults the high perseverators were rebellious, serious, shy and solitary, while non-perseverators were irritable, selfish, silent and anxious. Both he and Stephenson found among psychotics that melancholia, suspicion, paranoid states and some forms of dementia praecox were associated with very high perseveration, whereas hysteria and mania were connected with extremely low perseveration. There are some contradictions here and with our present results, which, however, may be partly due to the

alteration of the traits of perseveration with age and with the particular nature of the environment, *e.g.* the irritability of the high perseverator may be directed to the resistances of his own nature, and thus be a permanent feature of non-perseverative characters, whereas the irritation of the high perseverator may arise from the obstructions found in the restrictions and imperatives of society and therefore be a characteristic of perseverators only in unduly disciplined societies. The common core of agreement in our own and the above findings as to the shortcomings characteristic of extremely low and extremely high perseverators assigns to the latter a nature which is covertly rebellious, desultory, furtive, jealously suspicious, moody, melancholy, sentimental and vacillating. To the non-perseverator it assigns a nature which is inconsiderate, egoistic, irritable, impulsive (or 'headstrong'), demonstratively emotional and inconsequent. Some of these characteristics are not so much low '*w*' as just objectionable, but on the whole we can see why these subjects were voted to be of poor character. On the one hand (with low perseveration) conscientiousness, reliability and persistence in face of obstacles are impaired by a powerful wilfulness which chafes at the restrictions they impose and breaks through the inhibitions of the self-regarding sentiment. On the other hand (with high perseveration) the unreliability and lack of persistence springs from a general weakness and looseness of emotional organization which permits the whole personality to change according to the influences that are brought to bear upon it.

Essentially it would seem that low perseveration permits a well-knit conative make-up which is fundamentally stubbornly intransigent to external moulding influences and which tends to dissociate conative dispositions not fitting in with the main core. With high perseveration goes a less strong and more plastic nature which accepts inhibitions and imposed trends as part of itself and thereafter resents pressure. Internal conflict is tolerated; the sentiments are loosely organized and changeable, and the character is held together rather by some system of cognitive beliefs than by well-knit conative habits. In many respects the differences are such as might be derived simply from a greater abundance of nervous energy among non-perseverators.

Additional evidence on aspects of the perseveration difference was sought through an introspection questionnaire. Many writers, notably Otto Gross, have attempted to deduce all manner of temperament traits for high perseveration simply by imagining what the effects of perseveration (as known to introspection) would be. Such approaches we have seen to be grievously astray, firstly in associating high perseveration with

persistence of motive and secondly in identifying it with introversion. Is it possible that the error consists, not in associating introspectible perseveration with introversion, but in assuming that such perseveration is the same as that measured by tests? This we attempted to answer by means of a questionnaire. Lankes (9) has already shown that there is a correlation between test perseveration and the total score on a questionnaire dealing with all manner of introspectible perseveration, whilst certain test situations introduced among the present tests (notably 'attitudes') tap exactly the same processes as those subject to introspection. The following questionnaire modelled on Lankes's was scored item by item instead of as a whole, in order that any differences between various forms of perseveration might be made apparent. Each item was scored 3 for a very positive answer (with regard to perseveration), 2 for an answer indicating an indifferent state and 1 for a definitely negative answer. The subjects were divided into three groups with respect to perseveration scores—high, medium and low—and the total score of each group on each item of the questionnaire was worked out and set down as below:

	High 'p' (21 subjects)	Moderate 'p' (30 subjects)	Low 'p' (21 subjects)
(1) When you have made up your mind to do a certain piece of work do you sometimes find yourself persisting with it even after the causes for doing so have disappeared?	35	44	39
(2) In conversation can you pass quickly from one subject to another, or do you find it more natural to exhaust one subject at a time?	34	38	35
(3) When you have heard a tune does it sometimes recur again and again to your mind without your wanting it to do so?	56	51	49
(4) If you are in the course of tackling some problem do you find it difficult to drop it until you have solved it, or can you put it aside at any moment and take it up at some other time?	51	56	51
(5) If something goes awry early in the day, does it put you in a bad mood for the rest of the day or does your mood soon pass off?	40	39	37
(6) If you have heard two sides of a question does the one that you have heard most recently affect you more strongly than the remoter argument?	34	39	37
(7) If you are deep in thought and are suddenly asked a question, the answer to which you normally know quite well, do you have any difficulty in answering it?	51	52.5	48
(8) Do you sometimes carry out a task, <i>e.g.</i> an errand of some kind, and find that you have forgotten the reason for which you were doing it?	38	38	33
(9) Do you find much difficulty (<i>i.e.</i> are you confused) in doing two or three things at once, <i>e.g.</i> writing a letter and carrying on conversation?	42	51.5	38

	High 'p' (21 subjects)	Moderate 'p' (20 subjects)	Low 'p' (21 subjects)
(10) Do you start any activity, <i>e.g.</i> reading, playing tennis, without much warming up, or is there at first an interval during which you have to work yourself into the activity?	40	45	37
(11) When you are carrying out a task, <i>e.g.</i> reading, do you become quite oblivious to what is happening around you, such as being spoken to by people, or the playing of a piano?	54	51.5	48
(12) If an idea is expounded to you do you usually grasp it in its entirety in a single flash, or do you come gradually to an understanding of it?	39	44	41
(13) When you are travelling by train or boat do you continue to get the sensations of travelling for some time after the actual journey has ended, <i>e.g.</i> the persistence of the rhythm of the train wheels or of the motion of the boat?	36	31.5	32
(14) Are you much affected by travelling conditions, <i>e.g.</i> must you always face the engine?	25	27.5	26
(15) If you have some temptation or worry does it keep coming back again and again or can you keep it out of your mind for considerable periods of time?	48	51.5	50
(16) Are your dreams mostly about things that have happened or about entirely new things?	38	40	38
(17) Do you oscillate fairly quickly, when circumstances seem to dictate, between extremes of pride and sorrowful humiliation?	39	41	35

(All the figures in the middle (moderate) column have been increased by 1/20 to permit fair comparison with the other columns.)

Only four of the seventeen questions show a continuous rise from low to high perseverators, though none shows the reverse relationship. These concern inability to shake off moods (5), persistence of tunes (3), tendency to retract and restrict consciousness (absent-mindedness in all its forms) (8, 11), and, possibly, persistence of sensations (13). Some experiences are most keenly realized by moderate perseverators, and it is instructive to notice which these are. We find particularly: difficulty in dropping work after causes for continuing have disappeared (1), tendency to exhaust one thing at a time (2), difficulty in dropping a job till it is finished (4), difficulty in doing two or three things at once (9), consciousness of a warming up period in settling down to new activities (10), consciousness of effort in grasping a new idea (12), and others in which the difference is too slight to be of much significance. A discussion of these answers lays bare the fundamental weakness of the questionnaire method, namely, that the answers can mean very little because each respondent can have no idea of the standards in the minds of the other respondents. In addition one must reckon on some dishonesty, which affects answers to

questions dealing with undesirable traits, so that a high score may merely indicate more than average honesty. To say, for example, that moderate perseverators score high on the 'warming up period' question, may mean either that there is in fact a strong perseverative lag, or that the individual, having a strong will and high standards of bodily response to the will, has a more definite conflict between the will and the perseverative tendency, which, however, is no greater than or even less than that normally tolerated by less strong willed individuals. Finally it may mean nothing more than that the individual is more introspective and more conscious of all these processes than is a less observant man. Everything points to the second of these possibilities as being the true solution, for we see that perseveration as such, uncomplicated by willed activity, does associate itself with rise in perseveration score, whereas these qualities which associate themselves with the moderate perseveration score are all of a kind directed by the will. It is in good agreement with this conclusion that moderate perseverators have the highest rating for strength of will. We may therefore suppose that much of the discrepancy which is often reported between test scores for perseveration and the individual's estimate of himself for perseveration is due to a confusion of the persistence of willed activities with perseveration. To be unduly aware of inability to drop a job once started, or of resistance to doing two things at once or of a warming up period when starting a job one has decided to do, is to be regarded as evidence of good integration of sentiments rather than as evidence of perseverative tendencies. That these relationships are not shown more definitely by larger differences between the groups in their scores on the above questionnaire may reasonably be regarded as due to the attenuating effects of errors arising from the variation of standards from individual to individual. The rather large variations in the frequency with which the types of experience represented by the various questions are reported is itself of some interest, *e.g.* influence of travelling conditions and difficulty in getting off a subject of conversation are much less frequently reported than absent-mindedness when suddenly asked a question or obliviousness to all around when engrossed in reading.

V. TESTS OF SPEED, OSCILLATION, INTELLIGENCE, PHYSIQUE AND THE PSYCHO GALVANIC REFLEX RELATED TO THE 'c', 'w', 'a' AND 'm' TEMPERAMENT AND CHARACTER FACTORS.

A brief account has already been given of the lines of thought which led to the introduction of the other tests of character-temperament. It remains to give an abridged account of the exact conditions

of these tests before proceeding to examine the degree of success achieved. The battery includes a group of speed tests expected to correlate with surgency; a test of oscillation which might be expected to correlate with the suspected cyclothyme factor ('*m*') and with oscillation of mood; an intelligence test to determine whether '*c*' and '*w*' are independent of '*g*'; a measurement of the psychogalvanic reflex reaction which we thought would relate to certain surgent and will factors; and a rough index of physique to give indications of any relation between temperament and physique similar to that shown to exist by Kretschmer, and Sahai (14).

(1) *Speed of cognitive output.* The amount of purely reproductive and associative output appears to be high in well-marked surgent types. The subjects were instructed to write down as many words as occurred to them, as quickly as possible, for a period of 2½ min. (repeated a week later for reliability). In order to rule out assistance from intelligence or interference by the will in this spontaneous reproduction, and to break up long trains of association, the following conditions were imposed: (1) must not be all of one class, (2) must be two-syllable words, (3) must not be objects in the room.

(2) *Speed of writing.* Measured as letters or figures per minute in the *X* activities of all the '*p*' tests.

(3) *Speed of decision.* Subjects questioned (individually) without awareness of timing. Twelve standard questions, half on subjective, introspective matters, half on objective judgments. High intelligence or wide knowledge not demanded. Examples: "Do you feel more cheerful as a rule in the mornings or the evenings?" "Which of the two shades of blue on the card is darker?" (Twelve similar questions a week later.)

(4) *Speed of walking.* Told to walk at normal comfortable pace. Paces per minute on two different occasions.

(5) *Speed of reading comprehension.* (a) A complex and abstract passage (420 words), (b) a simple and dramatic passage (305 words), given to read at normal reading rate, silently, with prospect of a few questions on the matter read at the end. Repeated with similar passages a week later.

(6) *Oscillation.* Measured by number of reversals of perspective per minute when uncontrolled. Four 40 sec. periods with (a) the book perspective, (b) the three-cube perspective.

(7) *Intelligence.* Cattell Group Intelligence Test, Scale IIIA, 70 min.

(8) *Psychogalvanic reflex.* Apparatus as described in previous article (3). Subject presented with six visual word stimuli with a fair interval between each. Words to provoke a variety of instinctive impulses. Examples: 'Kiss,' 'Dog's vomit,' 'Deathly apparition.' Also two nooive physical stimuli, an unexpected crash of a biscuit tin and a knife plunged into table near subject's hand. It was not possible to repeat this test to measure reliability as each subject had to travel a long way to reach the laboratory.

(9) *Physique.* Records were kept of the subjects' weight, height and chest circumference (normal and expanded). The first physique index used (a rough approach to

Pignet's index) was obtained by dividing weight by height. This, however, really gives no true measure of adiposity or sturdiness. A better measure is obtained by dividing the weight by the cube of the height, since, apart from negligible variations of density, this fraction would be a constant for all people, of whatever size, having the same proportions (ratio of cross-section to height). Variations from normal proportions will therefore be revealed by this index, though we have no indication as to whether the variation is due to greater fleshiness or to broader skeletal structure.

The correlations given in Table V are calculated by the product-moment formula from the combined scores of the two or more applications of each test. We will first see how far the temperament and character measurements are independent of intelligence.

Correlation of intelligence with: Table V.

'c' or surgeny	-0.08 P.E. 0.09	Psychogalvanic reflex	-0.01 P.E. 0.09
		Speed of reading	0.01 " 0.09
'w' or character	0.16 " 0.08	Speed of decision	0.09 " 0.09
'p' perseveration	-0.14 " 0.09	Speed of cognitive output	-0.18 " 0.08

There are several recent findings of a related kind which suggest that the low positive correlation of 'g' with 'w' will be confirmed as a normal and regular relationship¹. Presumably higher intelligence makes character integration in a complex environment an easier matter, both by lessening the strains and suppressions which the individual is called upon to bear and by making more evident to him possible modes of sublimation. The low negative correlation with 'p' is also a normal but an undesirable feature of many present-day 'p' tests. For everything points to the conclusion that 'p' and 'g' are strictly independent, but that the Y activities in many tests, involving an eduction of correlates from the X activities, are in slight measure tests of intelligence. The only other correlation that may be significant—with cognitive output—indicates at least that the test gives no advantage to those who are more intelligent, indeed the low negative correlation would seem to suggest that the conditions imposed actually penalise the more intelligent by frustrating the habits of association which they are more likely to have acquired.

In Table VI are given the correlations of the new tests (intelligence test excepted) with 'c' and 'w.' Evidently the correlations with 'w' are negligible—we had expected nothing else—though their systematic negativeness suggests that very low but significant negative correlations would be obtained by a more extensive research. Whatever relation

¹ Woodworth (*Psychology*), for example, summarizes the evidence of the biographical approach by saying "on the whole those with higher intelligence were also of better character."

exists between 'c' and 'w' appears to be very slight and negative. Perhaps good character integration means a more complete utilization of nervous energy, so that rather less is available to express itself in the spontaneous channels characteristic of 'c' reactions.

Table VI. *Correlation of temperament tests with 'c' and 'w.'*

	Reliability coefficient	With 'c'	With 'w'
Speed of cognitive output	0.57	0.30 (P.E. 0.08)	-0.05 (P.E. 0.09)
Speed of reading	0.93	0.61 (P.E. 0.05)	-0.11 (P.E. 0.09)
Speed of decision	0.52	0.07 (P.E. 0.09)	-0.05 (P.E. 0.09)
Speed of writing	0.85	0.30 (P.E. 0.08)	Not calculated
Speed of walking	0.50	0.07	Not calculated
Physique. Index of weight/height	—	-0.09 (P.E. 0.09)	0.10 (P.E. 0.09)
Physique. Index of 'sturdiness'	—	0.06 (P.E. 0.09)	Not calculated
Psychogalvanic reflex (all deflections)	—	0.31 (P.E. 0.08)	-0.16 (P.E. 0.09)
Rate of oscillation	0.85	0.23	-0.04

It is with 'c' that the most significant and interesting connections appear. The correlation with speed of reading is so high as to suggest that this test might be forthwith employed as a clinical temperament test, comparable in validity to the intelligence tests of a few years ago. At the same time it must not be forgotten that this test result is obtained with a group of adults who have probably long reached their normal plateau in speed of reading. In other groups probably both age and 'g' would be partial determinants of speed, though no correlation of 'g' with speed of reading was found in this research.

The lowest significant correlation is with oscillation; and if we are to associate surgent with extravert and desurgent with introvert (for the traits ascribed to introverts are practically those of the desurgent temperament), the relation is in the opposite direction to that suspected by McDougall (who was using, however, less than half-a-dozen subjects). We began to doubt, too, whether this test was measuring any general tendency to oscillation. The existence of such a general tendency, moreover, rests on much less extensive evidence than that supporting a general 'p' factor, and the fields in which the tendency may be expected to appear are as yet little defined. A brief independent enquiry was therefore started at this point. Oscillation was now assessed by a questionnaire, scored on seven questions, dealing with such things as fluctuation in

emotional moods, in performance in games, in desire to work, etc. At the same time the actual tendency to vary was measured by the variation in each individual's score on the speed part of the 'p' tests from day to day. Finally we included the four judges' estimates on the liability of each subject to oscillation of mood. The intercorrelations of these four estimates of oscillation were all disappointingly small (see Table VII).

Table VII. *Measures of oscillation.*

		(1)	(2)	(3)	(4)
Reversible perspective	(1)	0.85	-0.08	0.15	-0.01
Variability on test scores (true oscillation)	(2)	-0.08	0.78	0.12	-0.05
Questionnaire	(3)	0.15	0.12	—	0.07
Estimates of mood oscillation	(4)	-0.01	-0.05	0.07	0.27

There is a hint that the questionnaire has really a slight positive relation to the other assessments. Is it possible that these various tendencies to oscillation are unrelated and that the questionnaire, because it taps a little of each of them, shows these slight correlations? We were forced to abandon the idea that the perspective test measured to any significant extent a tendency to oscillate, and we assumed, in view of later evidence, that it really measured mainly a speed function similar to that measured by the other speed tests.

Both the cognitive output test and the psychogalvanic reflex show a good connection with surgency. The connection is understandable in the case of the former, which is similar to and correlates with the other speed tests; but the psychogalvanic reflex has no obvious connection with speed and is known to be largely a measure of conation. It has already been shown to distinguish between individuals with respect to 'desire to excel,' 'rapidity of decision' and 'soundness of bodily constitution' (2); 'forcefulness of character' (3); success in examinations (but not 'g') (18); and 'general cheerfulness' (19), correlating positively with all of these to a significant extent. These results are fully in agreement with the present finding, for the traits are largely those found in the surgent temperament. This psychogalvanic reflex correlation with surgency is probably distinctly greater than the present figure would seem to indicate, for the measurement was made on only one occasion, and being the average of only eight stimuli probably has no very high reliability. The correlation has important implications for the nature of surgency, suggesting that it is not an absence of inhibition but the presence of greater conative vitality. It is interesting to notice that the correlation found by W. S. Brown (2) with speed of

decision, which was based on subjective estimates, is here confirmed by a correlation of 0.26 between the psychogalvanic reflex and objective measurements of speed of decision.

Apart from this measurement of conation by the psychogalvanic reflex, the highest correlations of surgency are with the speed tests, and the question arises as to whether it is to a general speed factor running throughout these tests that surgency is most intimately related. Hargreaves (7), in a more intensive study of the related trait imagination, showed that imaginativeness resolved itself into 'g' plus 'fluency of association.' The latter he rightly regarded as the central feature of the 'c' factor of Garnett (8). (It is this 'fluency' which we have sought to measure in the newly devised 'cognitive output' association speed test.) Working on test intercorrelations Hargreaves resolved this 'fluency' into three further factors—speed (as shown in ordinary speed tests), memory, and an ability which he was forced to call *x* because he could find no test which measured it in itself. The present tests were not designed for so thorough an analysis, our objective being sufficiently attained if we could show that tests of speed and fluency of association did in fact correlate significantly with the surgent temperament as directly estimated (*i.e.* not only as assessed in the related trait 'imaginativeness'). A glance at the correlations in Table VIII will show that all the speed tests are related and that the psychogalvanic reflex and oscillation tests behave as if they belonged among them.

Table VIII. *Intercorrelations of speed tests and other tests related to surgency.*

	Speed of reading	Speed of decision	Psycho- galvanic reflex	Cognitive output	Speed of walking	Oscil- lation
Speed of reading	0.93	0.17	0.21	0.21	0.18	-0.02
Speed of decision	0.17	0.52	0.26	0.16	-0.04	0.11
Psychogalvanic reflex	0.21	0.26	—	0.11	0.26	-0.18
Cognitive output	0.21	0.16	0.11	0.57	0.11	0.06
Speed of walking	0.18	-0.04	0.26	0.11	0.50	0.11
Oscillation	-0.02	0.11	-0.18	0.06	0.11	0.85

At the present stage of the work these results have not been analysed by the tetrad difference criterion, but it is clear from a brief inspection that the abilities (which are arranged in order of mean correlation) do not resolve themselves simply into a general factor and specific abilities without overlap. Neither is the above order the same as the order of correlation of the tests with 'c.' It looks as if the psychogalvanic reflex and Speed of Decision measurements have something in common, over and

above the speed element running through all the tests, which has no particular connection with 'c.' At the same time this general speed factor alone is not identifiable with 'c'; a pooled score of speed of reading, cognitive output, the psychogalvanic reflex and oscillation tests gives a correlation with 'c' of only 0.56 ± 0.086 , which is lower than that with speed of reading alone. When fuller data justify a more complete analysis the possibility must be borne in mind that though 'c' is a unitary factor from the point of view of behaviour it may not be so with respect to psycho-physical measurements. Hargreaves found that 'fluency of association' did not resolve itself into speed alone but into a speed factor, a memory factor and an α factor. Our results are by no means unfavourable to the view that 'c' on the behaviour side can be identified with 'fluency of association' on the measurement side, *i.e.* that surgency as a general behaviour factor arises from the combined action of three general psycho-physiological factors—speed of reaction, availability of memories and α . There is some suggestion from observation in the present enquiry that this α is an absence of inhibitions at deep levels.

The next step in trying out this notion is to devise more tests of fluency of association to find if more perfect correlations with surgency are obtained with the pool of such tests. At the same time we may grope towards better tests of 'c' by analysing the features of the speed tests which show the highest correlation with 'c,' and this may be done with the data of the present testing. This will give some idea, too, of the part which the speed factor alone plays in accounting for 'c.' In the data of the perseveration tests we have material which can be analysed in this way; for speed of complex (and long-established) habit can be scored from the letters test, speed of sheer mechanical movement from the triangles test, and speed of narrowly directed perception from the cancellation test. Contrasted with these more simple and stereotyped activities we have the performance in the attitudes test, which is a measure of a person's fecundity of ideas and rapidity of expression in response to a complex situation. The correlations of each of these with 'c' were as follows:

	Speed of writing	Speed of movement	Speed of perception	Speed of expression
Surgency with	0.28	0.08	0.04	0.32

The sharp distinction between the stereotyped perception involved in the cancellation test (looking for the same letter constantly) and the more complex perception, with rapid variation of the 'apperception masses' involved, which takes place in, say, reading, must not be overlooked if the above perception correlation is not to be regarded as anomalous.

Considered in conjunction with the results of Tables VII and VIII these figures clearly indicate that mere speed of mechanical movement has relatively little to do with 'c.' Speed of fairly complex habits of action or perception is more closely connected, while the closest connection is found with relatively complex activities involving memory, speed of re-arrangement of ideas and some spontaneity (or absence of inhibition?).

The test findings with respect to the 'm' and 'a' factors may be dismissed very briefly, for with the 'm' factor they were wholly negative, no significant correlation being found with any variety of test, while with the 'a' factor only a few indications were secured as to its affinities (see Table IX).

Table IX. *Test correlations with the 'a' ('adjustment' or cyclothyme) factor.*

	Per- severation	Intelli- gence	Psycho- galvanic reflex	Cogni- tive output	Speed of decision	Weight Height index	Sturdi- ness index	Oscil- lation
'a' factor with...	0.05	-0.07	0.17	-0.04	-0.05	0.13	0.12	0.30
P.E....	0.09	0.09	0.08	0.09	0.09	0.09	0.09	0.08

Here is no connection with either perseveration or the speed tests. The psychogalvanic reflex is partly involved, perhaps because greater conative energy makes good emotional adjustment more easily possible. The interesting connections are those with sturdiness (or the alternative physique index) and oscillation, since these are precisely the measurements which have been asserted to associate themselves with the cyclothyme constitution. Sahai, using a more comprehensive index of pyknic physique than this, found a correlation with cyclothyme characteristics scarcely any larger. The index obtained from weight and height alone he found to be only a very rough measure of the pyknic build. The best single index was found to be the facial index, but we did not know this at the time of starting the present research and we aimed, in any case, not so much at relating temperament to the pyknic build as to general adiposity. The most we can say of this section of the results is that an additional indication of the identity of the 'a' factor with the cyclothyme-schizothyme distinction is given by their common association with body build and oscillation.

VI. SOME PROBLEMS FOR FURTHER RESEARCH.

It seems advisable to set down here the following problems which have suggested themselves in the course of this research:

- (1) Confirmation is required of the conclusion that 'p' tests merely

involving an alternation of activities are not so good as those in which the alternation is between some old established habit and a similar but new habit. This has important consequences also for the theory of perseveration.

(2) In the latter case is it of much importance that the activities should follow closely one on the other, and in the former situation what is the best short time interval between the interfering activities, *i.e.* how quickly does the perseverative interference die down?

(3) Does the *X/Y* method of calculation effectively remove correlations with speed?

(4) Do the low correlations of motor perseveration tests with tests of perseveration of attitudes, feelings, ideas, and conative sets indicate that with better technique in these latter tests reasonable correlations will be obtained, showing perseveration to be a general factor extending to these introspectible experiences (when not obscured by willed action)?

(5) To what extent does perseveration vary with daily fatigue, physical condition and recent environmental influences? There was some incidental evidence in this research that though '*p*' may be to some extent constitutional it is capable of varying over a fairly wide range for the same individual in the course of some months.

(6) Are the exceptions to the close (non-linear) correlation of '*c*' and '*w*' merely due to poor perseveration test technique or is it possible for differences in training to effect marked differences in character integration among those with the same '*p*' score? With this problem is wrapped up the whole difficult question of establishing the real differences of high and low perseverators, in childhood, adolescence and the adult state.

(7) Although the most natural assumption is to suppose that extreme high or low perseveration produces poor character by some direct psychophysiological interference with ability to integrate the conative drives constituting the will, it is also possible that extreme perseverators are of poor character because they are temperamentally out of sympathy with the average, *i.e.* with the prevailing values. In other words, has perseveration an absolute significance for character or only one relative to the average of the group? Rangachar⁽¹³⁾ has shown, for example, that Jews have a higher perseveration than Englishmen. An English subject of high perseveration score (and therefore not of very good character integration) would be only a moderate perseverator in a Jewish community. Would he there be considered to show good character integration?

(8) Will the correlation of 'fluency of association' (speed and volume

of reproduced ideas) with 'c' prove to be complete apart from experimental errors of measurement, or is the correlation essentially a partial one, being upset by varying degrees of inhibition (imposed by early environment) acting upon the native vitality of which 'fluency of association' is a measure? Some marked exceptions to the relationship which were studied at first-hand in this enquiry suggest that the latter is the more probable state of affairs. Moreover, when corrected for attenuation, the correlations of 'c' with speed of reading and cognitive output only rise to 0.80 and 0.50 respectively.

(9) How far is fluency of association, like 'p,' subject to alteration through physiological and psychological influences?

(10) Further enquiry is indicated to confirm the independence of the 'a' factor with respect to 'c'; to extend the list of traits in which it is concerned; to prove its identity with or its relation to the cyclothyme pattern of temperament, and to discover more psycho-physical measurements with which it is related and by which it might be estimated. The results of the previous paper suggest that what is called the cyclothyme temperament is really high 'c' plus high 'a,' while schizothyme traits are low 'c' with low 'a.' High 'c' with low 'a' and *vice versa* have not been clinically noticed, so that the independence of 'c' and 'a' was not suspected, though the confusion and multiplication of types such as the introvert and schizothyme indicates that some awareness of a duality was present even among those making clinical observations.

VII. SUMMARY.

The following is a brief statement of the chief conclusions from the present research. Some can only be regarded as indications.

(1) 'c' and 'w,' the general temperament and character factors, are independent of intelligence; though there is an indication that the general character factor, 'w,' may have a slight positive correlation with intelligence.

(2) Perseveration appears to be brought out most strongly in tests which contrast an old established habit with some new arrangement of its elements and is hardly measurable in tests which merely contrast any free activities with the interference of those activities.

(3) Scoring tests by the X/Y formula reduces the intercorrelations but renders the 'p' measurement practically independent of speed.

(4) There are indications that test situations designed to gauge perseveration of conative trends, feelings, attitudes, etc. (such processes as

are designated perseveration in psychiatric work) measure the same perseveration as is measured by motor tests. The 'persistence' effect described by Cathcart and Dawson (3) is also probably of the same nature.

(5) Some forms of introspected perseveration—those involving the whole personality, *e.g.* finding it difficult to drop a task when in the middle of it—are indications of high '*w*' rather than of high perseveration and are found most among medium perseverators. Other forms, *e.g.* persistence of sensations, varieties of absent-mindedness, are related to the perseveration measured in tests.

(6) The surgent temperament factor, '*c*', is independent of perseveration but '*w*' is closely connected. Low '*w*' is associated with both extremes of the perseveration scale, but the correlation of '*w*' with '*p*' is negative because high perseverators have still lower '*w*' than have low perseverators.

(7) The differences between high and low perseverators are not of a very obvious nature. A tentative analysis has been made (p. 32).

(8) Surgency correlates positively with all manner of speed tests, from speed of walking to speed of reading. It is most highly correlated, however, with tests involving something more than the general speed factor, notably with 'fluency of association' as shown, for example, in speed of cognitive output and of reading. It is also related to strength of conative impulses as measured by the psychogalvanic reflex.

(9) The reversible perspective test of oscillation has only a slight connection with '*c*.' It appears to be more of a speed test than a test of oscillation.

(10) The '*a*' factor correlates neither with speed tests, nor with perseveration, but it shows some positive relationship to oscillation, to the psychogalvanic reflex and to a physical index of sturdiness (rough index of the pyknic body build) which agrees with the supposition previously made that '*a*' is probably the factor concerned in the cyclothyme-schizothyme distinction.

The writer wishes to express his great indebtedness to Mrs Monica Cattell for assistance in prolonged calculations, and to Messrs Kenyon, Porter, Theakstone and Reed for their care in making the assessments on which the first part of this work is based.

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(Manuscript received, 31 January, 1933.)

THE TENDENCY TOWARDS SYNTHESIS IN READING¹.

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I. INTRODUCTION.

THE experiment described in this paper was one of a number designed to investigate reading, and was carried out with the help of a grant from the Medical Research Council. Other experiments had shown that subjects who were occasionally disconcerted in reading short extracts sometimes indicated what they took to be breaks in the reading-matter. These imagined breaks suggested the present experiment, in which actual breaks were sometimes given.

¹ Parts of this paper were read before Section J of the British Association for the Advancement of Science at York, September, 1932.

II. EXPERIMENT.

A. PASSAGES FOR READING.

Ten passages were used. Those relevant are referred to in the footnote¹. They ranged from about 150 to about 250 words in length, and the first nine were constructed on the following plan:

First, fifth and ninth—single units of prose.

Second, fourth and eighth—double, each half from a different source.

Third, sixth and seventh—triple, an unrelated portion from a different source being inserted in an otherwise continuous piece.

The parts of the compound passages were chosen to give some contrast. When the experiment had begun, it was seen that the contrast was unnecessarily great. Then the fifth passage was dropped out for subjects I and J, and the tenth added. For these subjects the fifth was replaced by the ninth and the ninth by the tenth. Subjects C and H also read the tenth passage at additional sittings for that purpose. It was a paragraph into the middle of which was inserted part of a previous paragraph from the same composition. It gave more continuity than any of the other nine. All the material for reading was carefully chosen so that as many classes of prose and as many modes of contrast as possible were represented.

¹ No. 2. *The Psychology of an Infant*, by S. Bernfeld, Kegan Paul, 1929, p. 182, from "Oral Mastery has..." to "releases efforts of mastery." Then, *The Memoirs of Cardinal de Retz*, Everyman, II, p. 110, from "I beg leave of you..." to "solid and essential."

No. 3. *Journal of the Plague Year*, Defoe, Routledge, 1884, pp. 110-11, from "The Pain of the Swellings..." to "in the very Operation." Then, "College Librarians and their Work," *Times Educ. Supp.*, Oct. 18, 1930, p. 429, from "Few people..." to "are no readers." Then, Defoe continued to "wherever they found it."

No. 4. E. Cadogan, *Quarterly Review*, Oct. 1930, p. 226, from "The second principle..." to "is to be obtained." Then, *The Beautiful*, Vernon Lee, Cambridge, 1913, p. 102, from "Let us therefore..." to "the Third Dimension."

No. 6. *Essays in Criticism*, Matthew Arnold, Macmillan, 1925, First Series, p. 50, from "Let me remark..." to "essential part." Then, *The Elephant's Head*, Bartimeus, Cassell, 1930, p. 154, from "The torchlight..." to "fluttered about the clearing." Then, Arnold continued to "it will be subject."

No. 9. *Analytic Psychology*, G. F. Stout, Allen and Unwin, 1918, vol. II, p. 239, from "Whenever belief..." to "the ground on which we tread."

No. 10. *The Mediaeval Castle in Scotland*, W. Mackay Mackenzie, Methuen, 1927, p. 142, from "Now, from the early..." to "as a whole." Then, part of previous paragraph, from "They are assembled..." to "mass and disposition." Then, original continued to "bye, and the like."

B. SUBJECTS.

The eight subjects whose reports are utilized in this paper may be briefly characterized as follows:

Subject	Sex	Main interests
B	m.	Classics, philosophy, golf
C	f.	Art, music, plays
D	m.	Psychology, art, outdoor sports, politics, science
E	m.	Logic, mathematics
G	f.	Modern languages, plays
H	m.	Logic, literature, art
I	f.	Literature, art, psychology
J	f.	History, music, politics

C. METHOD.

The subjects were taken singly. They were seated comfortably, not facing the experimenter, in a quiet room, and each subject was directed to read aloud the passage on each card when it was handed to him by the experimenter, and then to report upon his process of reading, with any natural comments. Two readings were allowed on request. In most cases the experimenter timed the reading and the report. He made notes on the manner of the reading, and recorded the subject's report as nearly *verbatim* as possible. Great care was taken in making the records, and no doubtful reports are used in this paper. The quotations given are sometimes the relevant parts only of complete reports.

The nine passages were taken, for every subject, in three groups of three passages in each group, on separate days. Every subject thus had three sittings each lasting about 40 min., and subjects C and H each had an additional sitting for passage 10. Between the readings the experimenter talked to the subjects about indifferent matters.

III. OBSERVATIONS.

For the sake of brevity, the number of subject's reports here quoted in illustration of each point has been reduced to the minimum.

A. ON READING COMPOUND PASSAGES.

(a) *Complete synthesis.*

(1) Full synthesis without suspicion of breaks.

Subject H, Passage 10.

Experimenter's notes: "Fairly fast and smooth. No particular rhythm or emphasis. No change at break. Pause at second break. 1 min."

"Reads again, more fast, but carefully and with a readiness to pause and consider. Does not pause at first break. Long pause at second break. Pause at 'behind.' 2 min."

Subject's report: "Certain amount of visual imagery. I don't know why connected with Christchurch. Time suggests the two atrocities of All Souls'. Some of it I can't construe. Interested in trying to visualize buildings in place. Not clear what is meant by 'close tailing off behind.'" "A little difficulty about reconciling fact that main building is rather a space marked off by walls, and that it is the front of the construction. I can't visualize anything in the least like it, because it is not describing anything I've ever seen so far as I know. It's quite attractive; I shouldn't mind reading some more of the same. There I think I dry up. I'm interested enough to try to visualize the things described in place; though I get some imagery—Craigallion castle and the castle in Doune. And I should be quite glad to read general context. I'm not in the least interested who it's by." "I gather it was not broken up by irrelevancies." (4 min.)

Expectation of the passage being "broken up by irrelevancies" appears mainly in the subject's tendency to pause at the second break. The passage was taken as a whole, and the subject was not suspicious of breaks. Two observations may be made. First, there is a tendency to image "the things described in place." Relevant memory-images—of Christchurch, etc.—are revived. The subject's main interest in the passage is a tendency to visualization. Second, he is unable to get clear visual imagery of the "buildings in place." This he supposes to be because the author is "not describing anything I've ever seen so far as I know." The failure of imagery, however, is probably due to the breaks in reading-matter. In the whole process, therefore, are present the foundations for a grasp of the compound nature of the passage. Such a grasp might have arisen through the subject's attention becoming more and more occupied with the failure of the images that did arise to become concrete enough to fulfil the tendency to interpret the passage visually.

(2) Full synthesis, with explanations of peculiarities in the passage.

Subject D, Passage 2.

Experimenter's notes: "Starts loud and emphatic. Becomes quieter and mechanical. No change at break. Speeds up. Slightly sing-song. Quicker, voice tends to drop. Takes breath quickly."

"Wants it back as he didn't understand connection in first part."

"Again starts loud and slow and then voice drops and quickens up. Speeds up in second part. Slightly stumbling and hurried. Total, 3-5 min."

Subject's report: "I find difficulty in first part in understanding what it's about. It's taken out of context and I find it difficult to get a right attitude. I feel one would understand what it was all about if one had read the preceding passages. It gave the impression of being written in fairly modern times. But when he says, 'I beg leave to

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break,' it gave you the right attitude for a change of subject, and it goes from modern times and back to the seventeenth century. I don't get any clear idea of connection between first part and second part, and it wasn't any clearer the second time. If I had been reading silently I might have done better, as one breaks off and looks back, but you can't when reading aloud." (3 min.)

This might be described as a 'subjective' mode of explanation of the disconnection. An 'objective' mode can be illustrated too. Of the same passage subject I says:

"I can't see very much connection between first official part and the last part; there possibly isn't any except in the author's mind. It may be imaged or a process of free association."

Subject D gives the explanation in terms of his own change of attitude; subject I in terms of the writer's state of mind.

The report of subject D on passage 2 shows that he is baffled by a discontinuity which is almost obvious, but his tendency to treat the passage as a whole is strong enough to lead to the unwitting invention of excuses, which refer in part to his failure to read the passage aloud, and in part to his capacity for a change of attitude. The same tendency to treat the passage as a whole is shown by subject I. But she gives the explanation without reference to herself.

(3) Full synthesis, with marked criticism.

Subject G, Passage 2.

Experimenter's notes: "Much easier reading" than of the previous passage. "Soon becomes disturbed and clears throat. Careful over words. At breaks goes much quicker. More rhythm. Pronounces 'Parlement' in French. 2 min."

Subject G volunteered the following remarks after the first reading, and asked to be allowed to read the passage again:

"'Meer' is a misprint." "It is strange to follow the lines when there is no space. It worried me in the first half of that passage when the words were not usual."

Experimenter's notes on second reading: "Very careful as far as break, then goes much faster. 1.5 min."

Subject's report: "A great many words are not the right words in the right places — 'measures'. And there is a semicolon for a comma. And I don't understand at all what they mean by 'oral mastery' at the beginning. Is that from a book on psychology?—Yes it is isn't it? It is such a queer juxtaposition of matter altogether. And I think possibly 'historians' is not just the word he means either. Does he not mean persons writing home about Burmah, etc., not historians writing about past ages? You know how you get your eyes on a word below: that word 'Parlement,' I've a great objection to people who say French words in French, and yet I know I should stumble if I didn't pronounce it in French. I don't see why I shouldn't have said 'Parliament' and have done with it. There's no literary merit of any kind in that passage I should say, neither in choice of words nor in happening." (7 min.)

The subject accepts the passage as a single unit, but it has a quality which leads her to criticism. Yet, while the criticism might legitimately be an analysis of the disconnections, it tends to consist in objections of a dogmatic character—"Meer' is a misprint." Clearly her critical treatment is a natural attempt to understand the difficulties of the passage. But she does not realize that the passage is compound.

(4) Full synthesis with suspicions of the passage being compound.

Subject C, Passage 3.

Experimenter's notes: "Sounds interested. Reads with expression and rhythm. Quite fast. Hesitates at 'many died.' No pause at break, but tends to search. Goes on smoothly again past the (second) break. 1.5 min."

Subject's report: "I think it's awfully amusing, but what has the librarian in the middle to do with it? But I do like these gruesome things, Thucydides and Defoe, it reminds me of so many of these things. I wonder what it is that makes plague so fascinating to one. But actually the medical stuff amuses me too; the curious things they did. It's funny because I never wanted to be a doctor, but these things with a large admixture of image and complete disregard of the person's feelings...." (Pause.) "I can't help returning to that bit about the library. It has no quaintnesses and no capitals in it. Are you sure you didn't make up these extracts? It makes me extremely curious. It doesn't seem to fit in at all, to me. I want to make it fit in because it's printed between these bits about the plague and I've a feeling that it must be all of a piece. If you had put it in brackets I shouldn't have minded a bit, but I shouldn't have liked it nearly so much as the bits about the plague." (7.5 min.)

The subject hesitates and is uncertain. At first she tends to think the middle of the passage does not fit in with the rest; then she makes a general commentary on the first and last sections, which were obviously of a piece; then she thinks the middle part distinct; finally she treats the passage as a whole. There is a strong tendency to overcome this indecisive state of mind. Various lines of possible solution are attempted. First she is critical—the middle bit "has no quaintnesses and no capitals in it"; then a rationalization is tried, namely that the experimenter might have invented the passage. Later, after expressing her desire to unify the piece, she tries a way of testing the experimenter, to see if he will betray his suspected authorship. Tacitly assuming his authorship, she suggests that he should have put the middle bit in brackets. All these are natural attempts to account for, or set at rest, suspicions of discontinuity. The tendency to treat the piece as a whole is strong.

(5) Summary.

In the first of the above reports the passage is treated as a whole, and nothing is given but the expression of a tendency to stabilize the synthesis.

In the second it is again treated as a whole, but with the explanation of certain supposed difficulties. In the third, though it is still treated as a unit, a grasp of its compound nature tends to develop by way of critical analysis. But, since the essential condition of sound criticism—perception of the passage being compound—is absent, the criticism is misdirected. In the last report there is a fluctuation between a tendency to take the passage as a whole and a tendency to treat it as fragmentary.

(b) *Presence of tendency to synthesis despite perception of breaks.*

(1) Tendency to synthesis shown in delay of 'insight.'

Subject G, Passage 4.

Experimenter's report: "Reads a bit fast. No particular rhythm. Hesitates over several words. Pause at 'art criticism.' Then smiles. 1.5 min."

Subject's report: "I certainly don't understand the first half, and I don't understand the second. Is that out of a column in *The Times*? I'm afraid I mean that doesn't arouse anything in me. I don't want to say any more about that. Didn't amuse me in the least. I noticed I was taken in for about three words where the join comes before I noticed the change. It wasn't an abrupt change like the one yesterday. I mean it was abrupt but not so striking. At 'art criticism' I suddenly noticed it was one of those fake ones. I'd got there before I recognized it was a new [.....]¹ that there was a switch over. I was making a certain effort to follow the thread of the first passage and I thought it was going on into it; and I paused when I saw that it was a different bit." (5 min.)

The tendency to treat the piece as a whole arises through the "effort to follow the thread of the first" part. This effort gives rise to the subject's expectation that the "thread of the first" part "was going on into" the second. There is, apparently, nothing in the writing itself which leads to it being thought single, the tendency to do this being of purely subjective origin. The subject's reluctance to report—"I don't want to say any more about that"—may be due to her knowing that she was "taken in for about three words where the join comes." This indicates her attitude towards the tendency to treat the passage as a whole. She does not wish to admit its existence. She regards it as having betrayed her into error.

(2) Tendency to synthesis shown in treatment of the middle section as illustrative of the first part.

Subject B, Passage 6.

Experimenter's report: "No change at (first) break. Reads throughout slowly and with expression. Rhythm definite. Long pause at second break. Then goes on with expression and care, less rhythm. 1.5 min."

¹ Break in notes.

Subject's report: "Well there I found in the descriptive passage in the middle I was trying to see if there was an illustration of the first sentence. I thought there *was* going to be and I was disappointed that there was not a definite illustration. I hoped that it would suggest something about energy and honesty, but it didn't. The central passage was a very vivid description; I got quite a good idea of it. And again when he got to the end I thought perhaps that was going to be the key; perhaps it was something to do with pioneer spirit or something like that. But I wasn't able to fit it together at all. I thought there should have been a new paragraph at end of first piece and then you would take this as an illustration." He wonders whether the experimenter suggested to him that there ought to be a connection. "In the other case"—passage 3?—"it is two principles; but here it is a statement of a principle followed by a description. I think when there are two principles I would not be so likely to connect them as when there is a statement of a principle and then the description. The pause may be because I was looking forward to see the end of a long sentence."

The subject is satisfied that the sections of the passage do not fit together readily, and yet he searches for a means of connecting them. This searching takes the form of an attempt to treat the middle section as a concrete illustration of the principle in the first section. The tendency is so strong that even when the whole second section has been read and appears irrelevant to the first, he continues to look for a "clue" in the third section. At the end he expands his notion critically, with reference to a previous piece—passage 3? This report may be contrasted with those in section (a), in which the subjects treated the passages as wholes, and tended to be disturbed by irregularities. It may also be compared with the report just discussed, in which subject G tried to interpret the second part of passage 4 as a continuation of the first part. Here subject B distinguishes the parts, but searches for a principle of unity.

(3) Tendency to synthesis expressed in formal interpretation.

Subject C, Passage 6.

Experimenter's report: "Starts in preaching style. Smiles at break. Then a little faster and more natural. No change at (second) break except tendency to smile. Reads again in a preaching tone of voice: voice drops at end. 2 min."

Subject's report: "That's in symphonic form isn't it: it starts with a thing, has an interlude in the middle and goes back. I cannot grasp the heavy bits side by side with these purple patches. The middle bit just draws one's interest. The bottle and the arm made me think of that early Velasquez of the men drinking: a wonderful picture. I got a very clear image of the middle bit: I don't know what the other's about. I wish I could be really interested in morals and intellectuals." (3 min.)

The subject has no doubt of the passage being threefold—beginning, end and 'interlude.' But it is striking that she tends to find an explanation of this compound structure. She points out that the passage is in

"symphonic form," and in her definition of this form makes a slip sufficient to fit the form to the passage. The true comparison of the form of this passage with a musical form is with the simple 'ternary' form. In the ternary form there is a relieving section between two sections which may be identical. This form is not very unified, its interest often resting as much in contrast as in coherence. The subject, however, has chosen nominally to assimilate the passage to the symphonic form. In this form the 'interlude' has become a highly organized 'development' of part or all of the opening section. Thus the symphonic form has the contrast effects of the ternary form, together with a special unity depending upon a profound relationship of subject-matter in all three sections. Subject C is a musician, and would not be likely to make a downright confusion between the ternary and symphonic forms. However, she has thought of the passage in terms of the most unified form—the symphonic—and defined it in terms of the form which is at once less unified and more comparable with it—the ternary. The tendency to treat the passage as a unit is strong enough to make her deceive herself about the principle of unity adopted.

(4) Tendency to synthesis expressed in a circumstantial explanation.

Subject B, Passage 3.

Experimenter's report: "Starts fast. Expression. Slows down a bit. Laughs. Pauses and smiles at break. Laughs a good bit. Settles down. No pause at (second) break. 1.5 min."

Subject's report: "Plain sandwich. And what's more the second bit didn't follow directly on to the first section as there was nothing corresponding to the 'as above.' I couldn't decide what particular plague it was a description of though presumably it might be the Great Plague of London. Reason why I laughed was that it was such an absurd transition from the plague to the chatty remarks about the librarian. Struck me as the sort of thing that might happen through a mistake in binding a book. Not the sort of thing done deliberately. Or in a newspaper of course, that'd be the kind of thing. It was the kind of thing that was due to a mistake rather than deliberate because the connecting passage was omitted. I can imagine what the connecting passage would be like—how some people when being operated upon broke away and rushed down to the river. Also rather odd that one should have the description of the librarian and binding books coming into a passage where there was that sandwich."

The subject makes much effort to explain the inconsistency of the passage. He gives an explanation which involves him in the invention of a difficulty which does not exist. His explanation of the incongruous character of the middle section is that there has been an error in binding, or in some other way, such as getting the type muddled in the forme. But this explanation can be held only by presuming interchange between a

natural middle piece and the middle piece given. He finds no connection between the first and third sections. He says that there is nothing in the first section corresponding to the "as above" in the third section. But in fact the first and third sections are continuous, and the "as above" refers back to a paragraph not given. The subject has invented a break which does not exist, in support of his explanation of the break which does exist. This indicates a tendency to find unity in the passage.

(5) Summary.

The first report of this section shows mere delay in perception of the compound nature of the passage read. A complete prevention of this perception would lead to its being taken as a whole. The second shows a tendency to express the middle section as if related to the other sections in a rational manner, namely by way of illustration. In the third report the subject makes a formal synthesis which is largely cognitive, like all formal syntheses, but is not rational. She assimilates the structure of the piece to an irrationally modified musical form. In the fourth report the subject seeks an explanatory hypothesis to account for the breaks. He makes no attempt to find an internal synthesis. In the third and fourth examples the subject is led into secondary inconsistencies by the effort after unity.

(c) *Complete detachment.*

(1) Perception of the compound nature of the passage solves the subject's difficulty and gives satisfaction.

Subject D, Passage 6.

Experimenter's report: "Moderately fast. Hesitates at break and then goes on faster. Sounds interested. No change at next break, but slows down a bit. Sing-song at end. 1.5 min."

"Reads again for the sake of understanding it." "Much faster. Very careful at the end. Then speeds up."

Subject's report: "Well again obvious switch-over, which I was prepared for. Very marked change of subject (and) rhythm. Pedantic change for a pictorial style with some regard for rhythm. Second part was a successful attempt to hold up a visual picture. I saw all that visually. Wondering what it was about. Thought of Boer War. First section moderately clear. After second change passage meant nothing to me and I can't believe sentences were in original order, even if written by a moral philosopher. A glimmer of meaning. It seemed to me he would like a native to give up moral qualities. The word 'its' held me up. I abandoned the task of even partially understanding that part. There wasn't enough in the first part to say whether it meant anything or not. I can't have understood it. I'm glad I read it a second time as I wasn't sure whether it was my fault, but the second time I was satisfied it was complete nonsense."

The subject is confronted with many difficulties in understanding the passage at a first reading, and after a second, escapes all of them by interpreting it as consisting of incongruous sections. He also exaggerates his inclination to find the fault in the literary matter, by tending to regard the separate sections themselves as nonsense. He solves the whole problem of reading with understanding, by concluding that the passage cannot be understood. The effort to read is abandoned in such a way as to leave the subject satisfied that he has completed his task as well as could be expected. He was prepared for the "switch-over." But this simply made his solution easier. He expresses confident satisfaction at reaching his solution, saying, "I can't believe the sentences were in original order, even if written by a moral philosopher," "I abandoned the task of even partially understanding that part," and "I'm glad I read it a second time" for then "I was satisfied it was complete nonsense."

(2) Tendency to treat the passage as compound is exaggerated and gives amusement.

Subject E, Passage 2.

Experimenter's report: "Long pause before starting. Clears throat. Very careful of difficult phrases. Speeds up slightly at break. No other change. Monotonous. Goes back reading silently, quite a long time. Interjects, 'Parlement, spelt wrongly.' Pauses; reads very slowly. 3 min."

Subject's report: "Well that is a perfect jumble up of stuff and means absolutely nothing. It doesn't even suggest anything, except that (the) thing appears to be in three bits. First this about 'Oral m... something or other,' and... I've forgotten, and secondly this about historians, and then this about a prince or a duke. It leaves me with a sense of fun; it strikes me as just ludicrous. I wanted to laugh, especially at last bit. Not much connection between the first two bits. 'Oral mastery' was the word I was after." (3.5 min.)

The subject finds in the disconnections a solution of his difficulties. He expresses some satisfaction at this solution. But he exaggerates the disconnection. He speaks of the piece as a "perfect jumble up of stuff," and then divides it into three sections, whereas there are two only. This exaggeration is a natural tendency to make absolutely certain of the solution when found. And it may be an expression of satisfaction. The subject's wish to laugh at the 'jumble' supports the view that laughter or amusement is an automatic process tending to deflect difficulties.

(3) Attribution of success to convenient changes of attitude.

Subject D, Passage 3.

Experimenter's report: "Moderate speed. Dead sort of expression. No rhythm. No feeling for the swing of the passage. Voice drops at change and then speeds up.

Tries to be expressive. No break at (second) change, but hesitates a good deal. 2 min."

Subject's report: "That seemed obviously two passages, the first and last parts being continuous and the middle part quite disconnected and modern. The first part came from Defoe's diary of the Plague Year. I stopped having visual images because there would be much too much affect with it if one visualized all those horrible swellings. Then when it switched over it is fairly easy because first half of first sentence sets up a fresh attitude, and it easily slips back again into attitude of first part after. Imagery was very little. I began to visualize people jumping into river and being stopped by watch." (3 min.)

The subject refers the convenient changes of attitude to qualities of the passage, and suggests that its parts had been definitely put together in such a way as to awaken the necessary changes of attitude naturally in the reader. Undoubtedly this suggestion helped the reader to consider the passage more unified. Not every subject refers such changes of attitude objectively. Sometimes they are referred to his own ability to make them.

(4) Summary.

In the first example of this section the subject's discovery of the breaks is a solution of a difficulty in reading, and gives satisfaction. In the second the same general state of affairs exists, and the subject tends to exaggerate his complaints against the passage. In the third report natural and suitable changes of attitude are held responsible for success in finding the disconnections. The subject also tends to find unity, indicating that the parts may have been conveniently arranged to bring about these changes of attitude.

B. ON READING SIMPLE PASSAGES.

Subject J, Passage 9.

Experimenter's report: "Voice low and not so fast (as in the previous passage). Stumbles at 'having.' 1 min."

Subject's report: "I don't think it would make any difference if I read that again. It conveys nothing to me at all. The statements may be utterly contradictory. The final statement appears to forecast some sort of determinant view, but whether it is logical or not I don't know. Glad to notice 'Judgement' was spelt with an 'e.' I have to read it noticing the punctuation exceedingly carefully in order to make any sense of it at all. It seemed grammatically quite sensible. I didn't think there was any break in it, but I thought it quite possible it was drawn from two similar sources." (2 min.)

Here the higher grade cognitive syntheses are absent. The subject is able to identify the meanings of the separate words, and of the simpler

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forms of combination of words in the passage, for it seems grammatically sound, and the words appear to be used rightly and naturally. But she is not able to organize these simpler meanings into the system necessary for full understanding. Having observed that a number of the other pieces were compound, she suggests that this may also have been, as an explanation of its apparent discontinuity.

Other subjects make similar reports on reading the same passage.

Subject D reports: "Well! I can't understand that passage as a whole and I get no representation of its general meaning. The parts don't fit together in a whole." "Felt doubtful towards end whether sentences were in original order, or whether the experimenter had done some jumbling up."

Subject G reports: "Look here, it's not a consecutive passage, is it?" "I mean there are gaps." "I must confess the sentence I understand least is the last one, and that's made up of sentences. Am I right?"

These reports indicate the importance of a high grade cognitive synthesis, by which parts of a literary composition are apprehended in a united whole. Unifying apprehension is as important in reading natural units as in reading passages made up of fragments. Without it a literary work lacks full meaning, and a simple unit is as likely to be thought disjointed as is a compound structure.

IV. DISCUSSION.

A. SOME GENERAL REMARKS.

In normal reading-matter a complete break in rhythm, style, content, etc., will be clearly indicated either by beginning a new paragraph or by some kindred method. No such indications of breaks were given in the compound passages used in this experiment. In the absence of these indications subjects always tend to treat passages as unitary. This tendency is overcome only when the discontinuity is very great.

When a compound passage is successfully treated as a whole, mental processes, such as concrete imagery, tend to stabilize this synthesis. If difficulties are found they may be given automatic rational explanation, or set up a critical attitude. But grasp of the compound nature of a passage is needed for right criticism. Sometimes when a piece of writing is treated as a whole in general, there may be a slight and fluctuating tendency to treat it as fragmentary.

Frequently the subject has real understanding of the compound nature of a passage. Even then, however, the tendency to treat the matter as a whole often persists, and is expressed in various ways. It

may bring about a delay in perception of the breaks. The parts of the prose may be found by the subject to stand to one another in relations which seem to account for apparent disconnections. One subject thought the middle of a triple composition was an illustration of the first part. Another assimilated a compound passage consisting of three parts to a unified musical form. Explanation in terms of circumstance external to the writing may be sought. One subject attributed a peculiarity to an error in binding. Remarkable inconsistencies in an explanation may be overlooked in making it apply to a passage.

Often the literary material is accepted as compound. Then the discovery of breaks is generally a solution of a difficulty for the subject. He is released from the conflict set up by the tendency towards unification on the one hand, and the tendency towards discreteness on the other. The solution gives satisfaction. The relief from tension may lead to exaggeration of the disconnection found. The subject may be amused. But a further complication of the tendency to unification may develop. One subject found a passage compound, and attributed this perception to convenient changes of attitude in himself. But he was not satisfied with this, and attributed the ease of these changes to specially intended qualities of the prose.

Understanding a simple passage is dependent on unification of its parts. Great effort is also made to find unity in a compound piece. Evidently the tendency towards synthesis must be very important to comprehension.

B. SOME SPECIAL POINTS OF THEORY.

(a) *Concerning compound passages.*

(1) There is a strong tendency to treat a compound passage as a whole. Most of the compound pieces used in this experiment were made up of markedly different parts. Had the parts been easily connected, this tendency might not have been so striking. Frequently it is surprising that synthesis was effected at all. But no normal signals of breaks were given, and this must be taken into account. In normal reading there is no tendency to unite sections separated by commonly recognized indications, such as the indentation of a new paragraph. May it be that the tendency to synthesis found in this experiment was no more than the absence of response to normal indications of breaks? This is not possible, for the tendency to treat a passage as a whole when there is no break, cannot derive its explanation from the occurrence of the same tendency when there is a break. In fact, the use of conventional signs of breaks in normal

reading-matter points to the unifying tendency as primary in reading. Unless a reader tended to unify matter read, he would need no indications of breaks.

Subjects will go, automatically, to great lengths in the service of the unifying tendency. Many forms of image-synthesis, rationalizations, and hypothetical explanations of incongruities are found as expressions of this tendency. Subjects will deceive themselves quite unwittingly in accepting their own unsound explanations. Even when there is a grasp of the compound nature of a passage, the subject often persists in his automatic attempts to treat it as a unit.

(2) The tendency to treat a compound passage as made up of unrelated parts is also very marked. Like the tendency to unification, it is important in reading. This is shown by its taking the place of the tendency to unification wherever possible, and leading to satisfaction. A subject completes reading a passage either when he finds it seemingly fragmentary or when he finds it simple. If literary matter—compound or otherwise—can be interpreted as compound, the effort of reading is fulfilled. If it can be interpreted as simple, then, again the effort of reading is fulfilled. But if neither end can be gained, the subject tends both ways, and cannot complete the whole process of reading.

Of the two tendencies, however, that to treat a passage as a whole seems to be the more fundamental. The tendency to unification does not always cease when the breaks are found, but persists in subtle forms. Only when a passage cannot be unified is satisfaction gained from treating it as compound. Thus, though treating a piece as compound fulfils and terminates the process of reading, it is a negative process. Unless it brings the matter read under some wider scheme of synthesis, it amounts to giving up the attempt at understanding.

(b) *Concerning simple passages.*

(1) The secondary nature of the tendency to treat a passage as compound is well brought out by observing the reading of natural units. These the subject will admit to be fragmentary only when he completely fails to unify them. And then he will complain in no uncertain terms of their being incomprehensible. Evidently the tendency to unification is fundamental to reading. Indeed, meaning is primarily dependent on it.

(2) Synthetic apprehension of a simple passage gives meaning. The reader does not find meaning in apparently disconnected words or phrases. Then there may be meaning in each word or phrase, but none in the whole. Similarly, synthetic apprehension of a compound passage gives meaning.

Then the meaning is something created by the subject in his synthesis. But, in so far as the incongruities of a piece of literature can be explained or made apparently harmonious, it is given meaning as a whole.

Another, but less obvious, form of meaning is given by analytic apprehension. Meaning resulting from synthesis can be seen readily to consist in the form of the parts unified. Meaning resulting from analysis seems at first to consist simply in the fulfilment of a tendency to analysis. But in fact the meaning again lies in a form of synthesis. The words, phrases or sentences are found to be unconnected. The discovery of lack of connection is, however, a mode of apprehension of the relation between the parts. They are apprehended together as unrelated save in space and in time. The meaning of the unrelated parts then consists in this apprehension of them together. Meaning, therefore, again results from synthesis.

(c) *The "effort after meaning."*

These results bear a striking relation to Bartlett's findings in an investigation of perceiving and imaging¹. He concludes that in all acts of perceiving there is an "effort after meaning." This consists essentially in a tendency to escape the felt mental strain set up on the initiation of an act of apprehension. Strain is relieved by interpretation of the perceived material in accordance with the general character of earlier experience². He is led to the notion of an effort after meaning by observing (i) that there is a tendency for the mind to settle towards relatively stable interpretation of presented material, and (ii) that such interpretation is achieved by construction of that material into "organized settings"³.

In the present paper the expression "tendency towards synthesis" has been used where Bartlett used "effort after meaning," and it has been argued that meaning results from synthesis. Minute description and theoretical analysis have been avoided. It is hoped that Bartlett's views may be treated more fully when other experiments are being reported.

V. CONCLUSIONS.

1. In reading compound or simple passages there is a tendency towards synthesis of their parts. This tendency is expressed in various mental processes and in interpretations of the matter read. It may persist even after perception of the compound nature of a piece of writing is gained. It is

¹ Bartlett, F. C., (1916). *Brit. J. of Psych.* viii, 222-66.

² Bartlett, F. C., *op. cit.* pp. 231-2, 251 and 261-2.

³ Bartlett, F. C. (1932). *Remembering*, Cambridge University Press. p. 227, also pp. 20, 44-5, 84, 188.

primary in function in reading, and is the normal basis of the process of reading.

2. In reading compound or simple passages there is also a tendency to take them as fragmentary. This tendency is secondary in function. It is present normally and leads to criticism and understanding of the nature of a passage, but it develops fully only when the tendency towards synthesis is stopped.

3. Either tendency may lead towards meaning, and to the completion of the act of reading. By whichever tendency meaning is reached, however, it is ultimately a matter of synthesis.

(Manuscript received 9 September, 1932.)

SOME EXPERIMENTS ON FORMAL TRAINING IN THE ACQUISITION OF SKILL¹.

BY JOHN W. COX.

(*From the Laboratory of the National Institute of Industrial Psychology.*)

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I. THE PROBLEMS.

THE experiments described in this paper are part of a wider research which has been in progress for some time in the laboratory of the National Institute of Industrial Psychology. The research has been concerned with the general problem of the nature and measurement of ability at manual assembly operations. Brief reports have already been submitted to the British Association, dealing with such questions as how far the tests and other measures employed in this research are suitable and reliable measuring instruments, how ability at one test is related to ability at

¹ The substance of a paper read before Section J of the British Association, 1932.

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another and to general intelligence, how the improvement brought about by practice at a given manual operation ('improvability') is related to the initial ability shown by an individual, how this improvement is related to general intelligence, what are the mental processes underlying ability and improvability and, finally, to what extent improvement resulting from practice at one operation is transferred to other operations¹. During the past year, further work on the problem of transfer has been carried out and the present paper must be limited to this aspect of our enquiry. At the same time, the earlier work is not without bearing on the present results, since it has yielded much information relevant both to the practical employment of the tests here used and to the interpretation of the results.

The problem with which we are concerned may be generally stated as follows: how far does the training of one mental function improve other mental functions? The answer, however, may not be so simple and straightforward as the question suggests, since it may be affected by various factors. It may depend upon *the way in which the mental function has been trained*: one method of training may yield a transfer effect absent when other methods are employed. In the present experiments two methods were carefully distinguished. In one experiment the training consisted in repeating the operation at maximum speed for a given number of times. This method, which leaves the subject to his own devices, we shall call 'practice.' In the other experiment the repetition was accompanied by systematic instruction on points to be observed in manipulating the material. This method we shall refer to as 'training'².

The transfer of training may depend, again, upon *the nature of the function trained*, and upon its psychological relation to the other function to which transference is sought. It is conceivable that the transference may depend upon the relative complexity of the two functions, so that a transference occurs from a complex to a simple operation, but not conversely. Or it may depend upon the degree of similarity between the two operations, or upon the extent to which one operation forms a constituent part of the other. For this reason operations were chosen for experiment which exhibited these three psychological relationships.

¹ A full account of this work will, it is hoped, be soon published in book form.

² A similar distinction between 'practice' and 'training' had been previously made by Dr C. S. Myers who, in a paper on *Educability* given to this Association in 1928, wrote: "A fundamental distinction must be drawn between (a) the mechanical repetitive practice of an innate ability...and (b) that higher training which leads to the acquisition of the best attitude, the best technique and style and an adequate knowledge of general guiding principles enabling the best use to be made of an innate ability."

Finally, the answer may depend upon *the way in which the transference is effected*. This may occur either as an increase in ability or as an increase in improvability. If ability is increased, the effect of the practice or training at one operation will be to produce an immediate increase in ability at the other operation subsequently undertaken: if improvability is increased, the effect of practice or training may not show itself at once but will result in an increased rate of progress as practice at this subsequent operation is continued.

Before proceeding to the experiments themselves, one further point calls for attention, relating to the matter of measurement. All experiments on transfer involve the measurement and comparison of the improvement shown by different individuals (practisers and controls) at some given operation. In our examination of the curves of practice at these various manual operations, we found, of course, that the rate of progress as practice continued was by no means uniform, but tended to diminish as efficiency increased. In conformity with this tendency, individuals who ranked low on the scale of ability tended to make a greater improvement for a given amount of practice than those who stood initially higher; and this was true whether such improvement was measured in absolute units or as a percentage of the ability with which the individual started. It follows that any estimate of the effect of a given amount of practice or of training must take account of the position on the scale of ability at which the effect is produced. The effect is not necessarily greater in an individual whose score in speed is improved from 100 sec. to 80 sec. than in one whose score is raised from 40 sec. to 30 sec., but, if anything, the contrary. Only between persons of equal initial or terminal ability are such comparisons safely made.

II. THE OPERATIONS INVESTIGATED.

(a) *Assembling operations.*

The operations investigated were those involved in assembling and wiring an electric lamp-holder. After the subjects had familiarized themselves with the mode of assembly by carrying out the complete operation three times¹, the material was employed as manual tests as follows.

Screw test. Ten brass blocks of the kind found inside the lamp-holder were placed at random on the left-hand third of a sheet of foolscap placed horizontally before the subject, and ten small screws were placed at random upon the right-hand third. The operation consisted in picking up a block and a screw, inserting the screw into a hole

¹ Twice in the case of wiring.

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in the block, giving it one sharp turn to fix it, and placing the block (with screw now attached) on the middle third of the foolscap. The time taken to insert the ten screws constituted one trial; five such trials constituted one test¹, and were taken at one sitting.

Porcelain test. Here the time taken to assemble the pins, springs, large screws and blocks attached to the interior porcelain part of the lamp-holder (arranged before the subject in a definite order with the small screw already assembled) constituted one trial; five trials completed one test.

Container test. The operation consisted in attaching the porcelain to the two outer brass pieces by means of the ring which holds all three together. The additional ring which holds the lamp-shade in place was included in the case of adult groups of subjects only. Five such trials constituted one test.

Wedges test. Here the subject was required to insert the two wooden wedges at the top of the lamp-holder into the two brass parts into which they fit. As before, the material was arranged in a definite order, and five trials formed a test.

Wiring test. The whole of the various parts of the lamp-holder were arranged in definite order before the subject, together with a suitably prepared piece of wire and a screw-driver, the parts attached to the porcelain being already assembled thereto. The complete assembly and wiring-up of these parts constituted one trial; five trials formed one test.

(b) Stripping operations.

These employed the same material as each of the above tests, but the subject was here required to take the object to pieces and to sort out the parts under standard conditions. Thus in 'stripping screws' ten blocks with their screws already in place were laid in the centre of the foolscap sheet and the subject was required to undo the screws as quickly as possible, placing the screw approximately on the right-hand third of the sheet, and the block on the left-hand third. In the other operations the material was sorted into boxes. The same number of repetitions of the operation constituted a test as in the complementary assembling operation.

III. THE PRACTICE EXPERIMENTS.

These experiments fall into two groups. In the first group the aim was to discover how far the effect of practice at any one of the operations would be transferred to the other operations; in the second group, the aim was to investigate in similar fashion the effect of training, as distinguished from practice.

(a) Subjects.

The subjects for the practice experiments included both adults and schoolboys. The former were drawn from the staffs of the National Institute of Industrial Psychology and of the City of London College, the

¹ As thus administered, these tests were found to give sufficiently reliable measures of ability. Their reliability is described at length in an account of another research, especially directed to this question.

latter from the top two classes of an elementary school. The adults consisted of 39 practisers and 31 controls, the schoolboys of 38 practisers and 32 controls.

(b) *General plan.*

The general plan followed with our adult groups was to give every subject each of the assembling tests and the stripping screws test. These constituted his measures of initial ability. He was then placed into one of five groups. Four of these were practising groups; the fifth was a control group¹. Each practising group practised daily, for eleven days, one of the first four operations, in both its assembling and stripping forms. The practice began on a Monday, was omitted on Saturdays and Sundays, and continued until the following Monday week². A daily practice consisted in alternately assembling and stripping a given quantity of material for a given number of times under standard conditions. Thus the screwing group were supplied with twenty blocks and screws which they alternately assembled and stripped in groups of ten until the whole twenty had been assembled and stripped five times. The porcelain group assembled and stripped twenty porcelains daily in groups of five; the container group alternately assembled and stripped forty containers daily in groups of five; and the wedges group assembled and stripped fifty wedges in groups of five. On the day following the completion of the eleven practices, the subjects were again tested on the six operations for which they had been measured initially; these latter measures constituted the measures of terminal ability. The control group took the initial and terminal tests only, omitting the practice during the intervening period.

In the case of the elementary school groups, the general plan remained the same, but the procedure was modified by increasing the number of trials constituting the initial and terminal measures, and by including both the assembling and stripping forms of the operation throughout. The number of practising groups was reduced to two, each of whom practised *two* of the operations in both its assembling and stripping forms, and the number of daily practices was unavoidably reduced from eleven to five. By this plan we were enabled to extend the enquiry over a fairly wide range of ability and into a large variety of operations.

¹ It was necessary to have a control group in order to determine whether the improvement shown by the practisers in the terminal tests was a genuine transfer, or whether it was due merely to the additional practice involved in the terminal testing itself.

² With the exception of a few subjects who could not begin practice on a Monday but who conformed in other respects to the plan.

(c) Incentives.

In experiments such as these it is particularly important that the subjects should co-operate whole-heartedly, especially by conforming with the condition of working at maximum speed. We were fortunate in our elementary school, for the headmaster and staff took an active interest in the work, and this was reflected in the attitude of the boys who were obviously keen to do well at the tests, apart from any other incentive. At the same time, to minimize the chances of any flagging of interest and effort during the practice period, we gave a small monetary incentive based on (1) the subject's total score, (2) the number of times he beat his own best previous score, and (3) the number of times his group beat its own best previous score. The adults, some of whom were themselves trained psychologists, undertook the work voluntarily and were offered no special incentive beyond the encouragement of interest in the experiment itself. Observation of the subjects at work left little doubt regarding the genuineness of their effort.

It is beyond the scope of the present paper to discuss the relation of incentive and effort to ability, but it is noteworthy, (1) that no correlation was found between the scores made at the tests and the apparent effort displayed by the subjects as estimated by two independent observers, (2) that subjects frequently attributed a bad score to "trying too hard," and (3) that the boy who made the lowest scores throughout had personal reasons for wishing to do well and literally sweated at the work.

(d) Results.

(1) *Comparison of groups.* The adult groups were too small to provide data which could be profitably treated statistically¹, but it seemed worth while to compare, graphically, the initial and terminal scores of each group with those of the controls. Consequently the average score made at each of the five trials constituting the test (or total measure) was plotted as a graph. Thus charts were prepared for each group, similar to that for various combined groups shown in Fig. 1. These provided a ready means of comparing the initial and terminal performances of the practisers with those of the controls. On examination, it was seen that although the progress made by the practisers (*i.e.* the difference between their initial and terminal time scores) was markedly superior to that made by the controls in the operations which had been practised during the in-

¹ The 'transfer' problem was not the sole object of enquiry when collecting these data. It formed part of the wider enquiry referred to in the first paragraph of this article.

terval between the initial and terminal testing, no such superiority of progress was indicated in the terminal scores made at the other tests.

By suitably combining the groups we were enabled to obtain the average initial and terminal scores of a much larger group of subjects, all of whom had practised some (though not the *same*) operation other than

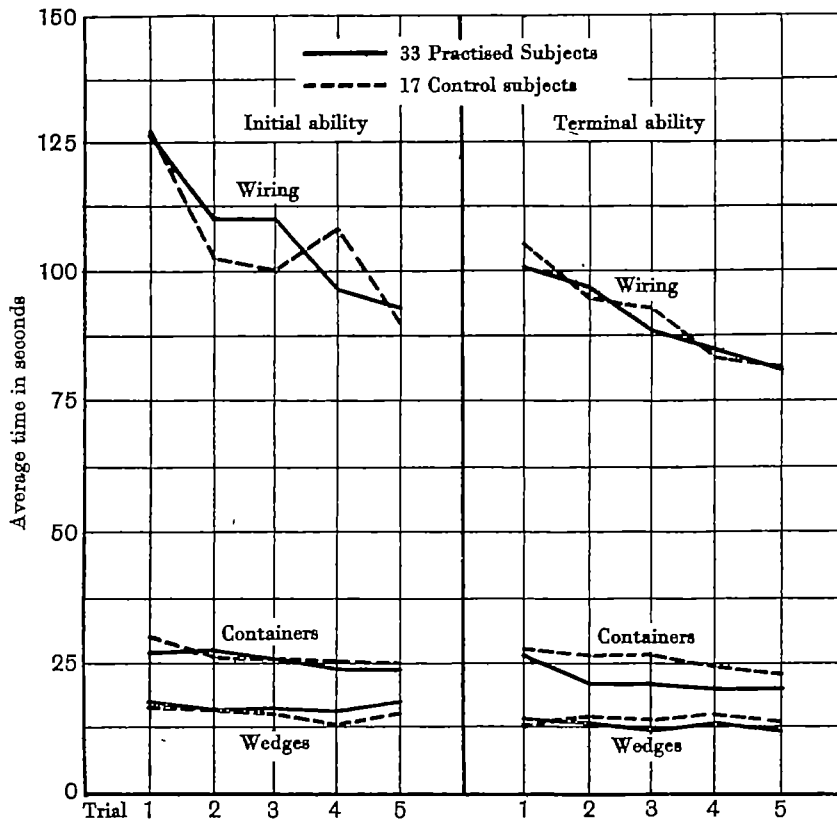


Fig. 1. Comparison between initial and terminal abilities of practised and control groups.

those for which they were tested terminally. The resulting scores are shown in Fig. 1. The five points on each graph indicate the average times for the five successive trials. On the left are given the average initial scores, and on the right the average terminal scores of all practisers except those who practised the operation in question. Thus the bottom right-hand practisers' curve (continuous line) gives the average scores

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made at the wedges test by all practisers except those who practised wedges, and similarly with respect to the other right-hand curves¹.

On comparing these curves with similar data derived from seventeen controls (shown in the same figure), it is seen that, with the possible exception of the curve for assembling containers, the curves of practisers fall into approximately the same positions on the chart as those of the controls, thereby indicating that the practice which these subjects carried out, and which resulted in a large and unmistakable practice effect in the operation practised, had no significant influence on the scores which they subsequently made at these other operations. Even in the curve for containers, the difference between the two groups are small—in no way approaching the magnitude of the differences observable in the curves of those who practised this operation, and they arise partly from terminal weakness in the controls rather than from any marked superiority in the practisers. Moreover, they are not confirmed in the data from the schoolboys, nor in the more detailed analysis of the data relating to the container operation which follows later².

A similar result emerged from the data provided by the schoolboys. Since these data were derived from larger groups, we were able to examine them statistically by computing the probable error of the difference between the improvement shown by the practisers (measured by the time taken initially minus the time taken terminally) and that shown by the controls. In only two operations did the improvement of the practised subjects exceed that of the controls: in wedges the excess was 7.8 sec. with a probable error of 5.8, and in porcelains the excess was 68 sec. with a probable error of 25.7. Neither of the differences is large enough in relation to its probable error to be important.

(2) *Additional data on container operation.* The operation in which our subjects were trained in the experiment to be described later was that of assembling containers. Consequently it seemed desirable to make a more detailed study of the effects of practice at this operation than was possible with the above-mentioned limited adult group, in order that a closer comparison with the effects of training might be possible. The number of adult practisers at containers was increased to thirteen and the number of controls to thirty-one, the same plan and procedure being followed as before. From these were drawn groups of equal initial ability by pairing,

¹ The terminal figures for wiring include all practisers, since none practised this operation.

² Similar results are observable in the curves of the other operations—*assembling porcelains, assembling screws and stripping screws*. Through limitations of space these are not given here, but may be seen on application to the writer.

for each operation, as many practised individuals as possible with controls making identical initial scores¹. Thus the groups were strictly comparable as regards both their total (or average) initial scores, and also the initial scores of the individuals composing them. The scores which were included within each group covered a wide range of individual ability.

The initial and terminal scores of these groups are given in Fig. 2²,

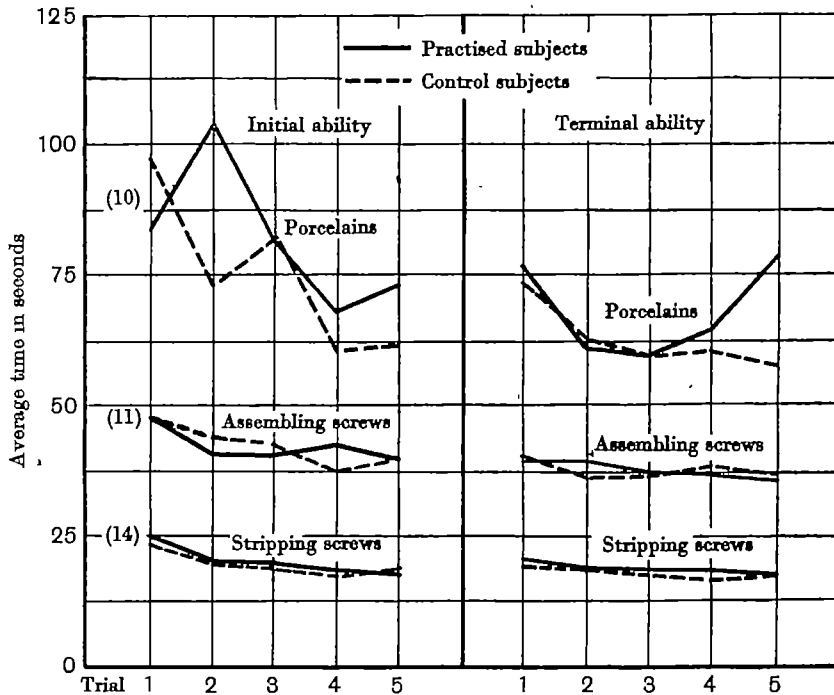


Fig. 2. Comparison between terminal ability of practised and control groups of equal initial ability. (Number of subjects in brackets.)

where the five points of the graph indicate, as before, the times taken to execute the five successive trials which constituted the test. The terminal graphs in these figures confirm our previous observations, since nowhere do they indicate a superiority of the practised subjects over the controls.

The same data are shown statistically in Table I. This gives, for each operation, (a) the average improvement made by the practised group (*i.e.* the mean value in seconds of the initial score minus the terminal score), (b) that made by the controls of equal initial ability, and (c) the standard

¹ Within a degree well within the limits of experimental error.

² For three of the operations only. See footnote 2 to p. 74.

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error of (d) the difference. Only in the case of wiring is there a difference of any magnitude in favour of the practisers, and this barely exceeds its standard error.

Table I. *Showing difference between mean gain of practisers and that of initially equal controls with standard error of the difference (seconds).*

Test	Practisers	Controls	Difference	Standard error
Assembling screws	23.3	22.7	+ 0.6	16.5
Stripping screws	8.6	9.5	- 0.9	5.7
Porcelains	50.4	86.3	- 35.9	31.6
Wedges	6.4	12.4	- 6.0	10.3
Wiring	73.9	30.1	+ 43.8	38.3

IV. THE TRAINING EXPERIMENT.

(a) *General plan.*

In view of this failure of transference of the effects of practice in one operation to others, it was of further interest to enquire whether or not there was any transfer of the effects of training. Consequently another experiment was carried out with the same general plan and conditions, with the exception that the high-speed repetitive practice which intervened between the initial tests and the terminal re-tests was now replaced by training, in which the subject effected far fewer repetitions of the operation, but, in the time thus saved, was given instruction in general principles and specific points relating to the best methods of carrying out the assembling operation. Thirty-six new subjects, similar in character to our former adults, were tested as before, trained on the container operation, and then re-tested. The eleven daily training periods were of approximately the same length as the former practice periods, and the initial and terminal testing took the same form as in the practice experiment.

(b) *The training scheme.*

The training was based upon an introspective analysis of the mental activities involved in carrying out these manual operations (which the writer had practised), and on observations made while watching the previous subjects at practice. The aim of the course was to impart a knowledge of certain general principles underlying the skilful handling of assembly material, and to provide specific exercise in applying them to *one* assembling operation, namely, the container operation. The scheme fell into two sections; in section I the general principles were explained,

and section II consisted of special exercises, based on the container operation, which the trainee was asked to carry out. These exercises aimed at showing how to apply the principles. They fell into five groups as follows.

The first group of exercises dealt with such matters of general method as the arrangement of parts on the bench¹, their order of assembly and the manner of holding them.

The second group might be called 'eye observation exercises': they showed what to observe through the eyes while engaged on the assembly operation.

The third group might be called, correspondingly, 'finger observation exercises': they indicated the things to be observed through the fingers. In the eye exercises the trainee was asked to look carefully at what was happening and to pay special regard to certain visual aspects of the shapes of the items and to the spatial relationship into which they should be brought. In the finger exercises the trainee was required to pay special attention to the 'feelings' in his fingers as they carried out the movements and to notice carefully certain aspects of this experience. Just exactly what to notice was made clear in each exercise.

The fourth group comprised exercises in the control of attention and of effort: they aimed at showing how these may be most economically employed throughout the operation.

The fifth and last group afforded practice in applying the results of the foregoing exercises to the operation as a whole under normal conditions of work.

This scheme was administered in the form of eleven lessons corresponding in time to the periods devoted to practice by our previous groups. Each lesson opened with a brief verbal revision of the chief points already dealt with. Attention was then directed to the 'point' of the next exercise, which was explained and demonstrated by the trainer. The subjects then did the exercise themselves, paying special attention to the point in question. Each exercise was repeated several times, the whole being treated as an observation exercise rather than one of mere speed. When all the exercises had been taken (by the eighth or ninth day, according to the group) the remaining days were devoted to revising the chief points and dealing with points (bad methods, etc.), observed in individuals during the speed tests with which each lesson concluded and which constituted the subject's daily measure of ability.

The speed tests consisted in first assembling and then stripping five containers, and were worked as follows: days 1 to 7, once; days 8 and 9, twice; days 10 and 11, three times.

¹ The matter of arrangement was included here for completeness but was not allowed to enter as a factor in the terminal tests. These were performed under the same external conditions as applied to the initial tests.

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Thus altogether the training consisted of talks, exercises based on the container operation, and 85 repetitions of this operation, as compared with the 440 repetitions which constituted the practice of our former subjects.

(c) *Results.*

(1) *The effect on ability.* As the data were obtained they were examined graphically as before. After the training, each of the trained groups showed a marked superiority over the controls in the various operations for which they were tested (tests of terminal ability). These differences were so large as to suggest at once that the effects of the training had transferred to the other operations. The issue was, however, complicated by the fact that some difference in ability was observable between the controls and the trained groups before the training was given (*i.e.* in the initial scores). To overcome this difficulty as before (see p. 75), we paired, with respect to each operation, as many trained individuals as possible with partners of equal initial ability selected from among the controls. Thus were obtained groups which were strictly comparable both as regards total and individual scores. The resulting graphs are given in Figs. 3 and 4, where it is seen that all the trained subjects are much superior to the controls with whom they formerly scored equally.

Since this terminal superiority, as shown in the graphs, is an average result, it remained to determine its statistical significance. This was done by computing how far the average terminal improvement shown by the trainees exceeded that of the controls, and the standard error of this difference. The results are given in Table II. In every operation except wedges the difference well exceeds three times its standard error; and in the wedges operation it closely approaches this standard. The terminal superiority of the trainees must therefore be considered as statistically significant.

Table II. *Mean gain of trainees and that of initially equal controls, with standard error of difference (seconds).*

Test	Trainees	Controls	Difference	Standard error
Assembling screws	36.9	11.0	+ 25.9	7.11
Stripping screws	33.8	8.7	+ 25.1	3.75
Porcelains	163.0	63.3	+ 99.7	27.96
Wedges	20.6	4.7	+ 15.9	6.12
Wiring	161.6	56.3	+ 105.9	32.4

(2) *The effect on improvability.* So far, our study of training has been confined to an examination of its effect on the ability shown in the

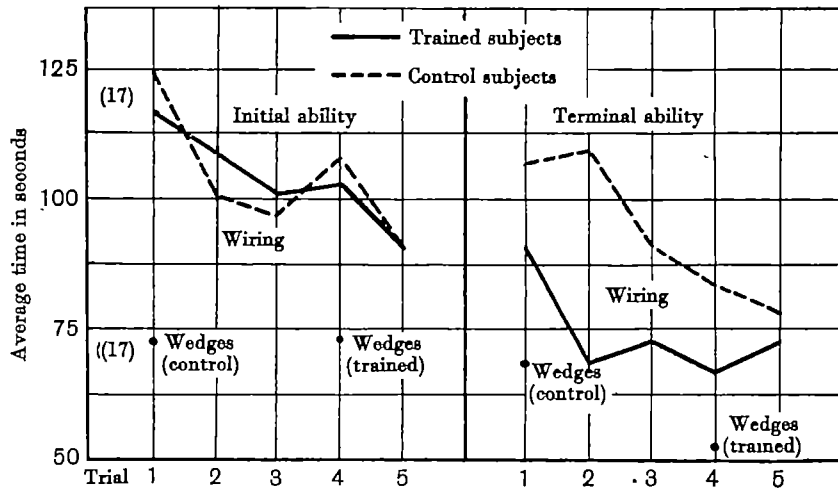


Fig. 3. Comparison between terminal ability of trained and control groups of equal initial ability. (Number of subjects in brackets.)

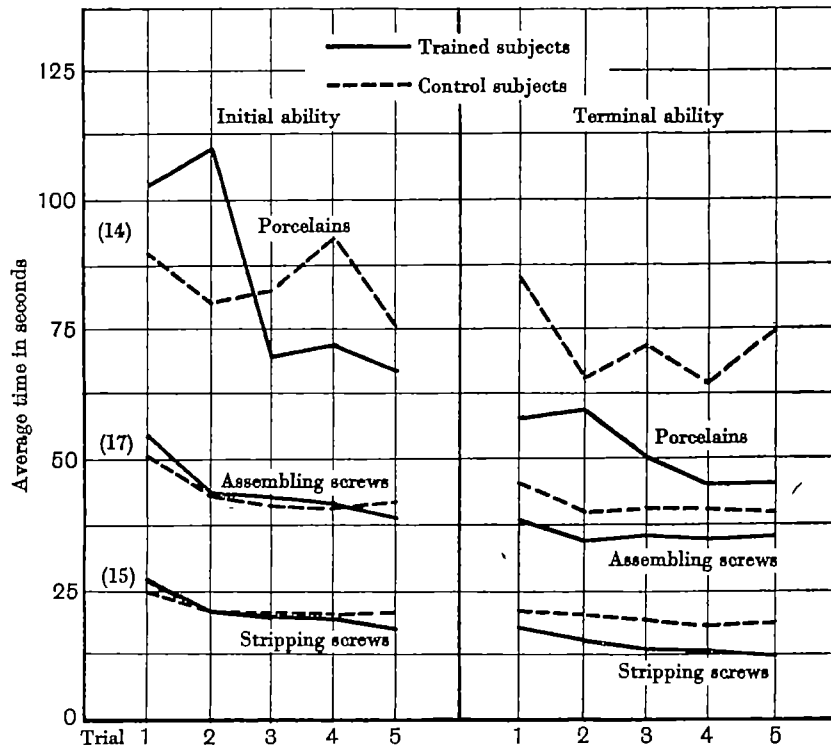


Fig. 4. Comparison between terminal ability of trained and control groups of equal initial ability. (Number of subjects in brackets.)

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terminal tests. We may now consider its effect on improvability—how far does training influence one's *rate of progress* at an operation subsequently practised? For this purpose thirty-five of our trained subjects practised the wedges operation under the same conditions as held for our former practisers (*i.e.* the untrained group). As the data were obtained, the results were plotted in the form of the graphs shown in Fig. 5. These show the average time taken to assemble five wedges (one trial) over each

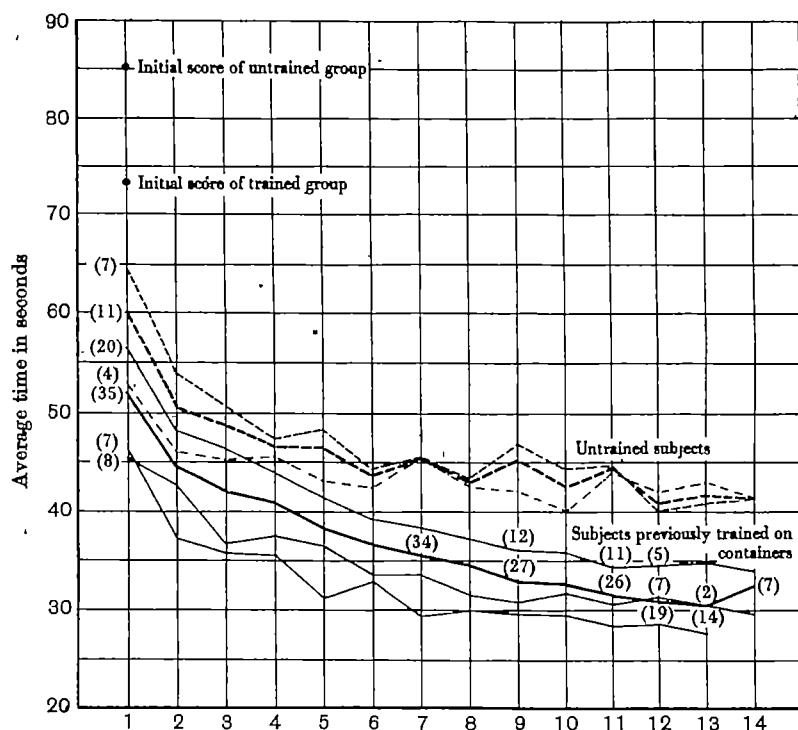


Fig. 5. Wedges operation. Graphs of trained and untrained groups.
(Number of subjects in brackets.)

successive half-practice (twenty-five wedges) by (a) three groups of subjects previously trained on containers and (b) two groups of untrained subjects. The graphs showing the results of combining the (a) groups and the (b) groups are indicated by deeper continuous and broken lines respectively. The untrained subjects number eleven throughout; of the trained subjects, thirty-five completed the first three days' practice (the first six points on the graph), after which the number who were able to continue the practice is given on the graph itself.

On comparing the graphs of trained with those of untrained subjects, the interesting fact emerges that, in general, the former take a steeper downward course, indicating a more rapid rate of progress, than do the latter¹. Whereas we have invariably found that curves of untrained subjects draw closer together as practice continues (as do their curves in Fig. 5), it will be seen that the curves of the trained subjects tend to draw farther away from those of the untrained subjects, owing to their more rapid descent. This means that not only is the rate of progress of the trained subjects greater than that of the untrained subjects actually

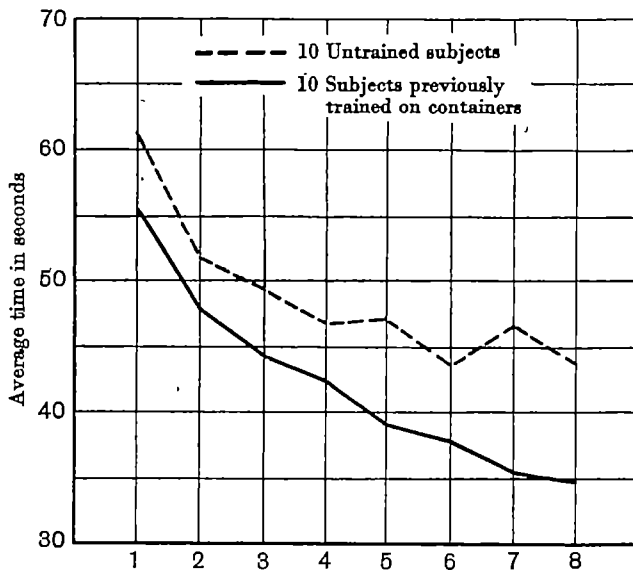


Fig. 6. Wedges operation. Practice graphs of trained and untrained groups of equal initial ability.

represented in the figures, but much more so than we should have reason to expect of untrained subjects who had attained to the speeds actually achieved at any point by the trained subjects.

While Fig. 5 enabled us to compare usefully all the data with respect to *relative rates* of improvement, it would be erroneous to suppose that the *absolute differences* in ability shown between the trained and the untrained groups as practice proceeds can wholly be attributed to the training factor, because these groups were not of equal ability at the beginning².

¹ The difference for the whole task (twenty-five wedges) at any point of practice is, of course, five times as large as the difference shown between curves in the figure, since the curves indicate the times taken to do one-fifth of this work.

² And similarly as regards the absolute improvement made by the groups.

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Valid comparisons of this kind are possible only between groups initially equal in ability. We have therefore paired off as many as possible of our trained subjects with untrained subjects of equal initial ability at the wedges operation (as measured by the wedges test which immediately preceded the practice at wedges). The practice curves of these initially equal groups, thus selected from the data of Fig. 5, are shown in Fig. 6, whence it is evident that the group which had received previous training excels over the untrained group from the first day of practice and maintains a higher rate of progress afterwards¹.

Although this result is based upon the limited number who could be thus paired as equal, it is not unprofitable to ask how far this observed difference between the two groups is statistically significant, for only in the light of such a statistical check can it be seen whether the difference is typical of the groups as a whole, or whether, on the contrary, it arises from the abnormal performances of one or two individuals. We have therefore determined the standard error of the difference between the gains (in time score) made by the groups over the first four days of practice (*i.e.* up to points 7 and 8 of the graphs). This proves to be 1.93 sec. Seeing that the difference itself (9.9 sec.) exceeds five times this value, it can hardly have arisen from the chance variations of individual scores. It seems most readily explained by the training on containers which the superior group had received before beginning this period of practice. The effect of this training seems clearly to have transferred in both a 'static' and a 'dynamic' sense, for not only are the trained subjects of superior ability at any given point of the curve, but their potentiality for further improvement under similar conditions of practice is greater.

(3) *Comparison between practice and training curves.* Finally, we may enquire how progress made during the training period compares with progress under conditions of practice. In so doing it should be remembered that the problem of transfer was the chief consideration when planning the training. Consequently this was not arranged with a view to the development of maximum efficiency at the container operation *within the training period*. The scheme aimed at instruction and illustration rather than at speed. The operation as a whole was repeated only 85 times during the training period, as compared with the 440 times during practice. For these reasons the training curve does not represent the efficiency that might have been attained had the training aimed at the production of mere speed in the container operation itself by a

¹ As before, the absolute time differences between the groups for the whole task (twenty-five wedges) is five times greater than the difference shown by the graphs (five wedges).

judicious blending of practice with instruction. At the same time, a comparison of the daily scores made by the trainees with those of the practisers is instructive in showing the effect of substituting in large measure oral and visual instruction for mere repetition.

The relevant curves are given in Fig. 7, where the heavy broken line indicates the daily scores of the practised group and the other broken lines show those of various groups of trainees. The latter are combined

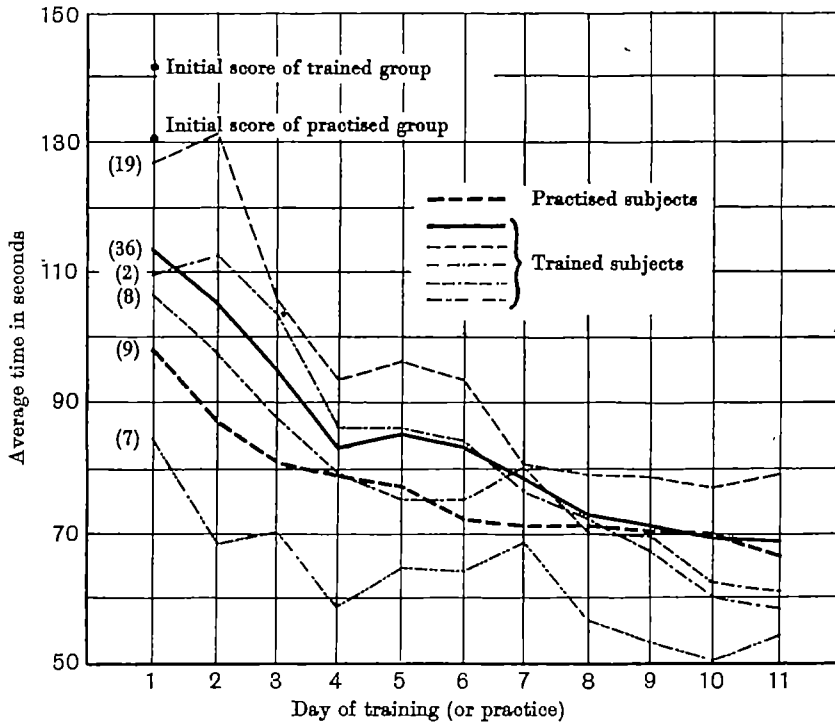


Fig. 7. Assembling containers. Comparison between practice and training curves. (Number of subjects in brackets.)

in the deeper continuous line which gives the average daily score for the whole thirty-six subjects. In every case the total progress of the trainees exceeds that of the practisers. A similar result is seen in the stripping curves, although in this simpler operation the difference between the two groups is less marked¹.

Fig. 8 shows similar data for groups starting 'initially' equal, selected from the data of Fig. 7 by pairing, as before, individuals initially equal.

¹ The stripping curves are not given for reasons stated in the footnote 2 to p. 74.

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The additional practice gives the practised group the advantage at first; but by the eighth day the trainees have caught up to them. By this time the practisers had repeated the operation 300 times, whereas the trainees had repeated it only 40 times.

A similar result is seen in the curves for groups initially equal at stripping containers¹. In this simpler operation the rate of progress after the first day is much the same for both groups, but the trainees never

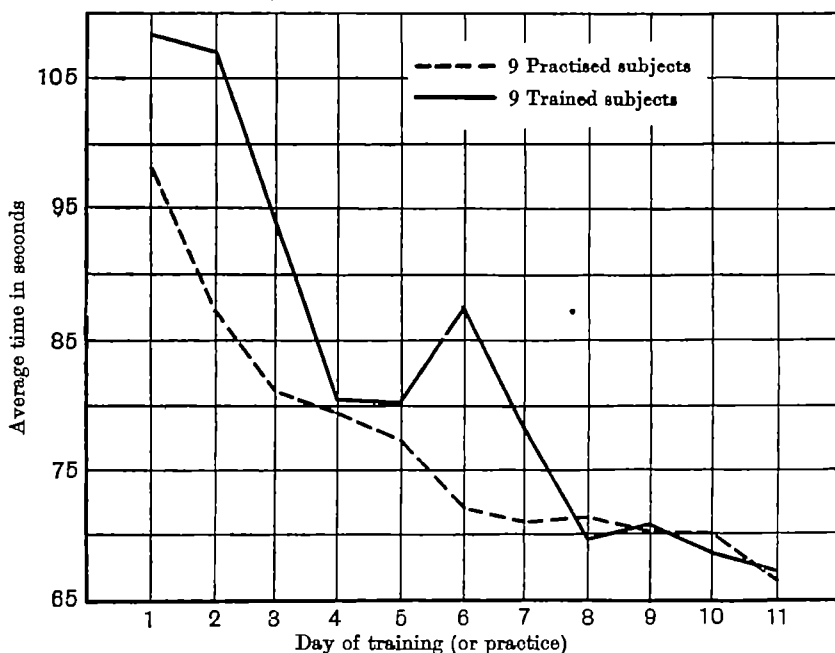


Fig. 8. Assembling containers. Comparison between practice and training curves for groups of equal initial ability.

quite recover from the advantage which the much larger number of repetitions gives to the practisers on the first day (forty repetitions as against one).

(4) *The rôle of intelligence in training.* It is an important practical consideration to ask how far the ability to profit by the training course may depend upon the trainee's general intelligence. This question shows two aspects: (1) how far the ability to understand and apply the ideas during the training period was an expression of the subject's intelligence; and (2) how far the ability to transfer and apply these ideas to the opera-

¹ See footnote to p. 83.

tion (wedges) which the trainees subsequently practised depended on their intelligence.

To answer the first question, all subjects took the test of the National Institute of Industrial Psychology for general intelligence (Group Test 33). The correlations of this test with total progress made during the training period were as follows: with absolute improvement at assembling containers, -0.05 ; with absolute improvement at stripping containers, -0.41 ; with percentage improvement¹ at assembling containers, 0.03 ; with percentage improvement at stripping containers, -0.38 .

It is at once clear, from the absence of positive correlation, that general intelligence has played no large part in determining progress under training. It should be added that the negative coefficients must not be taken to imply that the possession of intelligence was a handicap to progress in the stripping operation. They are readily understood in the light of the relations which we have invariably found to exist in our numerous practised groups between these functions (intelligence and improvement) and *ability*. Whereas intelligence tends to a small positive correlation with the ability which a subject initially displays at these operations, this ability itself tends to exhibit a marked negative correlation with the amount of improvement produced by subsequent practice². For this reason we should expect intelligence likewise to exhibit a negative relationship with improvement *in operations of this kind*.

To turn to the second question, the correlations of intelligence with the total progress made at the operations which the trained subjects subsequently practised were as follows: with absolute improvement at assembling wedges, 0.15 ; with absolute improvement at stripping wedges, -0.16 ; with percentage improvement at assembling wedges, 0.26 ; with percentage improvement at stripping wedges, -0.08 .

Here again, general intelligence can hardly be said to have been a serious determinant of progress, although it appears to have played a somewhat larger part than when directly applied to the training (in the container operation). Both here, and during the training period, the coefficients suggest the somewhat greater importance of intelligence in assembling than in stripping, and in training than in practice. It was our aim to present the training ideas as simply and clearly as possible, and perhaps the most interesting indication given by these coefficients is the

¹ *I.e.* the gain in time shown at the end of the training period over the initial score expressed as a percentage of the initial score.

² Expressed differently, the higher one is on the scale of ability the harder it becomes to make further progress.

small part played by intelligence in the assimilation and application of these ideas.

V. CONCLUSIONS.

Skill, developed by the mere repetition of one manual operation, confers little advantage on the performance of other operations that may be subsequently undertaken. Where, on the other hand, repetition is replaced by suitable instruction, the skill thus developed at no additional cost in time tends to transfer to other operations over a fairly wide range of manual activity. This transfer is manifested not only in superior ability, but also in a superior rate of progress. The advantage thus conferred by training was obtained, in the present experiments, without any loss of efficiency during the training period.

These results appear of great practical significance wherever work requiring manual skill is involved—especially when it is remembered that the limits of proficiency to be attained by training may far exceed those attainable by uninstructed repetition. These results indicate the wastage that must be produced by the usual practice of allowing beginners in the assembly room to drop into the work as best they can. And they suggest that a very real advantage would follow from the replacement of this current crude procedure by a short course of systematic training in the general principles underlying manual control, illustrated by specific examples from manual operations. A like procedure may be frequently adopted with advantage for other forms of manual activity, such as the work of our scholastic manual training centres, and 'coaching' for games, where often the so-called instruction offered resembles practice rather than training.

It is hardly necessary to add that, however important the effects of training may prove to be, they in no way negative the importance of enquiring into the effects of practice, for the two lines of enquiry, although similar, are not identical. What might have been transferred in practice need not be the same as what actually was transferred in training. A transference of practice effects would have suggested that the increased skill developed through practising one set of muscular co-ordinations had been transferred more or less directly to other series of co-ordinations in relative independence of the subjects' ideational activity. In other words, it would have suggested a transference on the neuro-muscular plane. The fact that no such transference was found even in our more complex operations suggests that the development of skill, when restricted to this neuro-muscular plane, is highly specific in its effects. On the other hand, con-

sideration of the training scheme and of the subjects' introspections suggests that the transference in the training experiments was effected at the ideational level—there was no general change in the lower neuromuscular mechanisms but rather an increased facility in dealing mentally with situations that involve the use of the muscles concerned.

To discuss this question in relation to the general motor factor which we found to run through these operations, and to general intelligence, would carry us beyond the scope of this paper. But if we ask, in conclusion, how far this transference of ideas is due to innate general intelligence and how far to knowledge, wisdom or experience, the reply would seem to be, to the latter, since little correlation was found between general intelligence and the improvement, absolute or percentage, which the trainees subsequently made at the wedges operation. That this transference depends less on innate intelligence than on use of the knowledge acquired during training leads to the interesting corollary that training of this kind should be effective wherever there is present the modicum of intelligence needed to understand the simple ideas involved in the exercises.

In conclusion, I would express my indebtedness to Dr C. S. Myers for valuable suggestions made throughout the research.

(Manuscript received 16 December, 1932.)

THREE SETS OF CONDITIONS NECESSARY FOR THE EXISTENCE OF A g THAT IS REAL AND UNIQUE EXCEPT IN SIGN.

BY H. T. H. PIAGGIO.

(From *University College, Nottingham.*)

- I. *Introduction and summary* (pp. 88-9).
- II. *Notation and definitions* (p. 90).
- III. *Analysis of the mathematical properties that g must possess, if it exists* (pp. 90-3).
- IV. *Definitions of g and s* (pp. 93-4).
- V. *Proof of Spearman's two-factor theorem* (p. 95).
- VI. *Necessary and sufficient conditions for these factors to be real* (p. 96).
- VII. *The necessary and sufficient condition for g to be unique except in sign* (pp. 96-8).
- VIII. *The effect of pooling tests* (pp. 99-100).
- IX. *Application to a dice problem* (pp. 101-2).
- X. *Application to psychological data* (pp. 103-5).
- References* (p. 105).

I. INTRODUCTION AND SUMMARY.

IN a short letter⁽¹⁾ entitled "The general factor in Spearman's theory of intelligence,"¹ I gave, without detailed proof, a method of obtaining explicit expressions for this general factor g and the specific factors. It was at one time asserted⁽³⁾ that g was unique if the tetrad relations were satisfied. This statement was challenged by E. B. Wilson⁽⁴⁾, and it has now been withdrawn⁽⁵⁾. J. O. Irwin⁽⁶⁾ has shown that if one obvious slip in Wilson's expression for g is corrected, the result is equivalent to that given by me. B. H. Camp⁽⁷⁾ uses geometry of N dimensions, a formidable subject even to mathematical specialists. Another treatment has been given by H. B. Heywood⁽⁸⁾, but this also is addressed to mathematicians. It is not surprising that a standard treatise⁽⁹⁾ should state that "to probe the foundations of this theory... may need mathe-

¹ My article "Mathematics and Psychology" in the *Mathematical Gazette* (2) is a sketch of Spearman's theory for mathematicians with no knowledge of psychology.

mathematical aptitude and training of high order." However, I believe that this is no longer the case, and that apart from the novelty of some of the results, one of the merits of the treatment in the present paper is that it uses only elementary mathematics and the simplest parts of the theory of correlation, such as are familiar to every psychologist who works with mental tests. It is hoped that sections VII and X may be of practical value in preventing the useless expenditure of much time and energy, for they show that a single very good test does more towards fixing g than all the other tests combined.

The chief novelty in this paper is the prominence given to certain conditions, in addition to the tetrad relations, which must be satisfied before the two-factor theory is applicable. Two conditions are necessary to ensure that the factors shall be real. These are usually satisfied in practice, but there is a further condition necessary if g is to be unique except in sign. The ambiguity in sign (which does not seem to have been noticed previously) is easily explained in the case which occurs most commonly, but in general its interpretation is not clear. After a discussion of the effects of pooling tests, the conditions of sections VI and VII are exemplified by a dice problem (suggested by the work of G. H. Thomson); g exists and is real, but its numerical value is indeterminate. Finally we examine the actual sets of mental tests collected to support the two-factor theory, and we find that no set completely satisfies the condition for numerical uniqueness of g , though recent sets do so more nearly than the older ones. Thus it appears that the numerical uniqueness of a general factor in mental tests has not yet been demonstrated, but that the evidence in support of it is becoming stronger. On the mathematical side there seems little remaining to be done. At one time some doubt existed as to the validity of the fundamental mathematical theorems, but the identity (in my opinion) of the conclusions reached by Wilson, Camp, Heywood, and myself¹, using different methods, encourages the hope that these results are now firmly established. As for the probable error of the tetrad differences, formerly another controversial matter, the treatment by J. Wishart⁽¹⁰⁾ has not been challenged.

¹ I started from an algebraic transformation of Spearman's theorem (*loc. cit.* p. v, line 13 to p. vi, line 12). This proof of the existence of g when the tetrad relations hold is the vital part of the mathematical side of Spearman's work, and I do not agree that it is unsound or "indicated without giving a formal proof." Moreover by the words " $i \equiv$ any new variable uncorrelated with all the others" Spearman was the first to show that g is indeterminate.

II. NOTATION AND DEFINITIONS.

We shall use A to denote briefly a set of N numbers $A_1, A_2, \dots$, such as those obtained by the application of the same mental test to N persons. $\bar{A}$ is used for the arithmetic mean, and the differences $A_1 - \bar{A}, A_2 - \bar{A}, \dots$ are denoted by $a_1, a_2, \dots$ respectively, so that the arithmetic mean $\bar{a}$ is zero. Except when otherwise stated, in all the following work the numbers $a_1, a_2, \dots$ will be supposed to be divided by such a common factor as to reduce their standard deviation $\sigma_a = (\sum a^2/N)^{1/2}$ to unity. This is what is meant by *expressing the set of numbers in standard measure*.

Now take K such sets, say $A, B, \dots$, each consisting of N numbers, and try to express them in terms of $(K+1)$ other sets $G, S_A, S_B, \dots$, no two of which are correlated, and of $2K$ constants $m_a, m_b, \dots, n_a, n_b, \dots$, by equations such as

$$a = m_a g + n_a s_a \quad \dots\dots(1),$$

$$b = m_b g + n_b s_b \quad \dots\dots(2).$$

Each equation such as (1) is an abbreviation for N equations, as a can take any one of the N values $a_1, a_2, \dots$, with a corresponding set of values for g and s_a . (But m_a and n_a are constants which occur unaltered in each of these N equations.) It will be convenient to use the term *averaging* to denote the process of taking the arithmetic mean of the expressions of a given type. Thus by averaging a, a^2 , and ab we get respectively $\bar{a}, \sigma_a^2$, and $\sigma_a \sigma_b r_{ab}$, where r_{ab} is the coefficient of correlation between the sets A and B . As we have used standard measure, these three averages reduce to 0, 1, and r_{ab} respectively.

III. ANALYSIS OF THE MATHEMATICAL PROPERTIES THAT g MUST POSSESS, IF IT EXISTS.

As a preliminary to the formulation of our definition of g , we investigate the relations which must be true if equations such as (1) and (2) hold good. Equating the product of the left-hand members of these equations to the product of the right-hand members we get

$$ab = m_a m_b g^2 + m_a n_b g s_b + m_b n_a g s_a + n_a n_b s_a s_b,$$

whence, averaging and remembering that g and the s 's are mutually uncorrelated,

$$r_{ab} = m_a m_b \quad \dots\dots(3).$$

Similarly

$$r_{ad} = m_a m_d \quad \dots\dots(4).$$

Hence

$$r_{ab}r_{ca} = m_a m_b m_c m_d.$$

Interchange of the suffixes b and c does not affect the right-hand member of this equation, hence

$$r_{ab}r_{ca} - r_{ac}r_{ba} = 0 \quad \text{.....(5)}.$$

This is the well-known *tetrad relation*. Similar relations must hold for every four suffixes.

Again, from (1), $ag = m_a g^2 + n_a s_a g,$

hence, averaging, $r_{ag} = m_a.$

Similarly $r_{bg} = m_b.$

Hence, using (3), $r_{ab} = m_a m_b = r_{ag} r_{bg} \quad \text{.....(6)}.$

Using this and similar results for r_{ac} and r_{bc} ,

$$(r_{ab}r_{ac}/r_{bc})^{\frac{1}{2}} = (r_{ag}^2 r_{bg} r_{cg}/r_{bg} r_{cg})^{\frac{1}{2}} = \pm r_{ag} \quad \text{.....(7)}.$$

From (7), r_{ag} will be real or imaginary according as $(r_{ab}r_{ac}/r_{bc})$ is positive or negative. Equation (6) shows that r_{bg} will be real if r_{ag} is so. Similarly $r_{ag}, r_{bg}, r_{cg}, \dots$ are either all real or all imaginary. Equation (7) also shows an ambiguity in the sign of r_{ag} , but from (6) the sign of r_{bg} is fixed when that of r_{ag} is given, and similarly for $r_{cg}, \dots$. In the tests quoted in Section X, $r_{ab}, r_{ac}, r_{bc}, \dots$ are all positive, and hence $r_{ag}, r_{bg}, \dots$ are real and either all positive or all negative.

Squaring each side of (1)

$$a^2 = m_a^2 g^2 + 2m_a n_a g s_a + n_a^2 s_a^2,$$

hence, averaging, $1 = m_a^2 + n_a^2,$

and $n_a^2 = 1 - m_a^2 = 1 - r_{ag}^2 \quad \text{.....(8)}.$

Now choose multipliers $w_a, w_b, \dots$ such that the coefficient of correlation between g and the combined test $t = \Sigma w_a a$ may be a maximum, where t is to be in standard measure.

Averaging the relation $tg = \Sigma w_a ag,$

we get $r_{tg} = \Sigma w_a r_{ag} \quad \text{.....(9)}.$

Also averaging $t^2 = \Sigma w_a^2 a^2 + 2\Sigma \Sigma w_a w_b ab,$

we get $1 = \Sigma w_a^2 + 2\Sigma \Sigma w_a w_b r_{ab};$

so, using (6), $1 = \Sigma w_a^2 + 2\Sigma \Sigma w_a w_b r_{ag} r_{bg},$

i.e. $1 = (\Sigma w_a r_{ag})^2 + \Sigma \{w_a^2 (1 - r_{ag}^2)\} \quad \text{.....(10)}.$

From (9) and (10), $1/r_{tg}^2 = 1 + [\Sigma \{w_a^2 (1 - r_{ag}^2)\}/(\Sigma w_a r_{ag})^2] \quad \text{.....(11)},$

so r_{tg}^2 is greatest when $\Sigma \{w_a^2 (1 - r_{ag}^2)\}/(\Sigma w_a r_{ag})^2$ is least.

Some writers have attempted to obtain the maximum value of r_{tg} by equating to zero its partial differential coefficients with respect to $w_a, w_b, \dots$. However, such a method gives a value that may be a maximum, or a minimum, or neither. To avoid this uncertainty we proceed as follows.

From the identity

$$\begin{aligned} [\Sigma \{w_a^2 (1 - r_{ag}^2)\}] \left[\Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} \right] - (\Sigma w_a r_{ag})^2 \\ = \Sigma \Sigma \left[\frac{w_a r_{bg} (1 - r_{ag}^2)^{\frac{1}{2}}}{(1 - r_{bg}^2)^{\frac{1}{2}}} - \frac{w_b r_{ag} (1 - r_{bg}^2)^{\frac{1}{2}}}{(1 - r_{ag}^2)^{\frac{1}{2}}} \right]^2, \end{aligned}$$

of which the right-hand side is a sum of squares, it follows that the least value of either side is zero. Dividing each side by

$$\left[\Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} \right] (\Sigma w_a r_{ag})^2,$$

which is positive, when $r_{ag}^2, r_{bg}^2, \dots$ are all positive and less than unity, we see that the least value of

$$\Sigma \{w_a^2 (1 - r_{ag}^2)\} / (\Sigma w_a r_{ag})^2 \text{ is } 1 / \left[\Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} \right] = \frac{1}{S}, \text{ say,}$$

so from (11) the greatest value of r_{tg} is

$$(1 + 1/S)^{-\frac{1}{2}} = \{S/(1 + S)\}^{\frac{1}{2}} \quad \dots\dots(12).$$

This maximum is attained when

$$w_a (1 - r_{ag}^2) / r_{ag} = w_b (1 - r_{bg}^2) / r_{bg} = \dots = \lambda, \text{ say } \dots\dots(13).$$

From (10),

$$1 = \lambda^2 \left[\left\{ \Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} \right\}^2 + \Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} \right] = \lambda^2 (S^2 + S) \quad \dots\dots(14).$$

From (13), the greatest r_{tg} corresponds to $t = \lambda \Sigma \frac{ar_{ag}}{1 - r_{ag}^2}$, and of the

two signs for λ given by (14), the positive makes $w_a r_{ag}, w_b r_{bg}, \dots$ all positive, giving r_{tg} positive. The negative sign for λ makes r_{tg} negative, giving the minimum instead of the maximum.

Now make an estimate of g in terms of t by using the regression equation

$$(\text{estimated } g) - \bar{g} = (\sigma_g r_{tg} / \sigma_t) (t - \bar{t}).$$

As we are using standard measure, this reduces to

$$\text{estimated } g = r_{tg} t,$$

or, using (12) and (14),

$$\text{estimated } g = \left(\frac{S}{1 + S} \right)^{\frac{1}{2}} \lambda \Sigma \frac{ar_{ag}}{1 - r_{ag}^2} = \frac{1}{1 + S} \Sigma \frac{ar_{ag}}{1 - r_{ag}^2} \quad \dots\dots(15).$$

Now put $g - tr_{tg} = ki$ (16),

where k is a constant chosen to make $\sigma_t = 1$.

Squaring and averaging (16),

$$1 - 2r_{tg}^2 + r_{tg}^2 = k^2,$$

so from (12),

$$k^2 = 1/(1 + S).$$

From this and (16),

$$g = \frac{1}{1 + S} \left\{ \Sigma \frac{ar_{ag}}{1 - r_{ag}^2} \right\} \pm \frac{i}{(1 + S)^{\frac{1}{2}}} \text{(17).}$$

From (1), (8) and (17),

$$s_a = \pm (1 - r_{ag}^2)^{-\frac{1}{2}} \left[a - \frac{r_{ag}}{1 + S} \left\{ \Sigma \frac{ar_{ag}}{1 - r_{ag}^2} \right\} \mp \frac{ir_{ag}}{(1 + S)^{\frac{1}{2}}} \right] \text{(18).}$$

The significance of the terms involving i occurring in equations (17) and (18) is discussed in section VII.

These equations also show that g will be imaginary if $r_{ag}, r_{bg}, \dots$ are so, and s_a will be imaginary even if g is real, if r_{ag}^2 is greater than unity. These cases were excluded in our determination of the maximum value of r_{tg} . Again, there is an ambiguity¹ in the sign of g , due to the ambiguity already mentioned in the signs of $r_{ag}, r_{bg}, \dots$. In the usual case, when $r_{ab}, r_{ac}, r_{bc}, \dots$ are all positive, and consequently (as already proved) $r_{ag}, r_{bg}, \dots$ are all real and of the same sign, the value of g corresponding to positive values of $r_{ag}, r_{bg}, \dots$ seems to indicate something in the nature of general ability, and the value of g corresponding to negative values of $r_{ag}, r_{bg}, \dots$ something in the nature of general lack of ability. But in the more general case when $r_{ag}, r_{bg}, \dots$ are real but not all of the same sign, it is difficult to recognise which value of g indicates ability and which lack of ability. As for the ambiguity in the sign of s_a , due to the $\pm$ before the factor $(1 - r_{ag}^2)^{-\frac{1}{2}}$, it seems clear that the positive sign indicates specific ability for the test a , and the negative sign the lack of this ability.

IV. DEFINITIONS OF g AND s .

We now investigate the converse of the results obtained in the last section. Starting from a number of tests² which satisfy the tetrad relations (without any of $r_{ab}, r_{ac}, r_{bc}, \dots$ being zero), it follows that

$$r_{ab}r_{ac}/r_{bc} = r_{ab}r_{ad}/r_{bd} = r_{ac}r_{ad}/r_{cd},$$

so the value of $(r_{ab}r_{ac}/r_{bc})^{\frac{1}{2}}$ is independent of b and c , and depends on a alone. We denote either of its two values (positive and negative) by α .

¹ First mentioned in my letter to *Nature* (11).

² It is usual to insert the adjective *dissimilar* before *tests*, but we omit it for the reason stated in section X, where we discuss how to select five tests from Kelley's nine.

When the existence of g has been established it will follow, as in Section III, that α is the same as r_{ag} , but it seems illogical to use a symbol referring to g before g has been defined. Similarly we use β for what will later prove to be the same as r_{bg} , and so on. The sign of β is to be fixed in terms of that of α by the equation, corresponding to equation (6),

$$r_{ab} = \alpha\beta \quad \dots\dots(19).$$

Similarly we define $\gamma, \delta, \dots$ and fix their signs in terms of that of α .

We now *define* g and s_a by what equations (17) and (18) become when $r_{ag}, r_{bg}, \dots$ are replaced by $\alpha, \beta, \dots$ and where i denotes an *indeterminate* set of N numbers (reduced to standard measure, like $a, b, \dots$) of which all that is postulated is that it is uncorrelated with any of the sets $a, b, \dots$.

It will simplify the subsequent work to show that these definitions give g and s_a in standard measure. Averaging the modified forms of equations (17) and (18), we see that the arithmetic means of g and s_a both vanish. Squaring the modified (17) and averaging, we get,

$$\sigma_g^2 = \frac{1}{(1+S)^2} \left[\Sigma \frac{\alpha^2}{(1-\alpha^2)^2} + 2\Sigma\Sigma \frac{\alpha\beta r_{ab}}{(1-\alpha^2)(1-\beta^2)} \right] + \frac{1}{1+S}.$$

But from (19),

$$r_{ab} = \alpha\beta,$$

and

$$2\Sigma\Sigma \frac{\alpha^2\beta^2}{(1-\alpha^2)(1-\beta^2)} = \left\{ \Sigma \frac{\alpha^2}{1-\alpha^2} \right\}^2 - \Sigma \frac{\alpha^4}{(1-\alpha^2)^2} = S^2 - \Sigma \frac{\alpha^4}{(1-\alpha^2)^2},$$

so

$$\sigma_g^2 = \frac{1}{(1+S)^2} \left[\Sigma \frac{\alpha^2 - \alpha^4}{(1-\alpha^2)^2} + S^2 \right] + \frac{1}{1+S} = \frac{S+S^2}{(1+S)^2} + \frac{1}{1+S} = 1.$$

Similarly, squaring the modified (18) and averaging, we get

$$\begin{aligned} \sigma_{s_a}^2 &= (1-\alpha^2)^{-1} \left[1 + \frac{\alpha^2}{(1+S)^2} \left\{ \Sigma \frac{\alpha^2}{(1-\alpha^2)^2} + 2\Sigma\Sigma \frac{\alpha\beta r_{ab}}{(1-\alpha^2)(1-\beta^2)} \right\} \right. \\ &\quad \left. + \frac{\alpha^2}{1+S} - \frac{2\alpha}{1+S} \left\{ \frac{\alpha}{1-\alpha^2} + \frac{\beta r_{ab}}{1-\beta^2} + \dots \right\} \right] \\ &= (1-\alpha^2)^{-1} \left[1 + \frac{\alpha^2}{(1+S)^2} \{S+S^2\} \right. \\ &\quad \left. + \frac{\alpha^2}{1+S} - \frac{2\alpha^2}{1+S} \left\{ \frac{(1-\alpha^2)+\alpha^2}{1-\alpha^2} + \frac{\beta^2}{1-\beta^2} + \dots \right\} \right] \\ &= (1-\alpha^2)^{-1} \left[1 + \alpha^2 - \frac{2\alpha^2}{1+S} \{1+S\} \right] = 1. \end{aligned}$$

V. PROOF OF SPEARMAN'S TWO-FACTOR THEOREM.

Eliminating i from the modified (17) and (18) we get

$$a = \alpha g \pm (1 - \alpha^2)^{\frac{1}{2}} s_a \quad \text{.....(20).}$$

Squaring (20) and averaging,

$$1 = \alpha^2 \pm 2\alpha (1 - \alpha^2)^{\frac{1}{2}} r_{gs_a} + (1 - \alpha^2),$$

$$\text{so} \quad r_{gs_a} = 0. \quad \text{Similarly } r_{gs_b} = 0 \quad \text{.....(21).}$$

From (20) and a similar equation for b , by multiplication,

$$ab = \alpha\beta g^2 \pm \beta (1 - \alpha^2)^{\frac{1}{2}} g s_a \pm \alpha (1 - \beta^2)^{\frac{1}{2}} g s_b \pm (1 - \alpha^2)^{\frac{1}{2}} (1 - \beta^2)^{\frac{1}{2}} s_a s_b,$$

whence, averaging and using (19) and (21),

$$r_{ab} = r_{ab} \pm (1 - \alpha^2)^{\frac{1}{2}} (1 - \beta^2)^{\frac{1}{2}} r_{s_a s_b},$$

$$\text{so} \quad r_{s_a s_b} = 0 \quad \text{.....(22).}$$

Similarly the coefficient of correlation between any other pair of s 's vanishes.

It may be well to emphasise that in sections IV and V there is no assumption of linearity of regression or of the possibility of expansion by Taylor's theorem. Section III certainly contained a regression equation and so assumed that the regression was linear, but this section was merely preliminary and inserted to *suggest* the definitions in IV and V. From a purely logical point of view section III (including the determination of the maximum of r_{ig} and the conditions thereby imposed on $r_{ag}^2, r_{bg}^2, \dots$) could have been omitted without affecting our definitions or proof.

The nature of the definition of g may become clearer if we consider the special case when all the coefficients of correlation $r_{ab}, r_{ac}, r_{bc}, \dots$ have the same positive value r , giving $r_{ag} = r_{bg} = \dots = \pm r^{\frac{1}{2}}$, and $S = Kr/(1 - r)$, where K is the number of tests. Equation (17) becomes

$$g = \pm \frac{r^{\frac{1}{2}} (\Sigma a)}{1 - r + Kr} \pm \frac{(1 - r)^{\frac{1}{2}} i}{(1 - r + Kr)^{\frac{1}{2}}} \quad \text{.....(23).}$$

For large values of K this gives the approximate equation

$$g = \pm \frac{1}{r^{\frac{1}{2}}} \left(\frac{\Sigma a}{K} \right) \pm \frac{(1 - r)^{\frac{1}{2}} i}{K^{\frac{1}{2}} r^{\frac{1}{2}}} \quad \text{.....(24),}$$

i.e. for large values of K the determinate part of g is in this case approximately a numerical multiple of the arithmetic mean $(\Sigma a/K)$.

VI. NECESSARY AND SUFFICIENT CONDITIONS FOR THESE FACTORS TO BE REAL.

The discussion following equation (7) in section III shows that g will be real if the signs of r_{ab} , r_{ac} , r_{bc} are all positive, or if one is positive and two are negative. If all three are negative, or if one is negative and two are positive, g is imaginary. To construct an example of the second case, take

$$\begin{aligned} r_{ag} &= -\sqrt{(-1)}, & r_{bg} &= 0.8 \times \sqrt{(-1)}, \\ r_{cg} &= 0.5 \times \sqrt{(-1)}, & r_{dg} &= 0.4 \times \sqrt{(-1)}. \end{aligned}$$

These give $r_{ab} = r_{ag}r_{bg} = 0.8$ and so on, leading to the table of correlations (satisfying the tetrad relations)

	a	b	c	d
a	—	0.8	0.5	0.4
b	0.8	—	-0.4	-0.32
c	0.5	-0.4	—	-0.2
d	0.4	-0.32	-0.2	—

Similarly the discussion following equation (18) shows that s_a will be real when g is real if r_{ag}^2 does not exceed unity¹.

As

$$r_{ab}^2 = r_{ag}^2 r_{bg}^2$$

and r_{ab}^2 cannot exceed unity, only one, at most, of r_{ag}^2 , r_{bg}^2 , ... can exceed unity, so only one, at most, of s_a , s_b , ... can be imaginary. To construct an example, take

$$r_{ag} = 2, \quad r_{bg} = 0.4, \quad r_{cg} = 0.3, \quad r_{dg} = 0.2,$$

giving the table of correlations (satisfying the tetrad relations)

	a	b	c	d
a	—	0.8	0.6	0.4
b	0.8	—	0.12	0.08
c	0.6	0.12	—	0.06
d	0.4	0.08	0.06	—

To sum up, *the necessary and sufficient set of conditions that g , s_a , s_b , ... may all be real is that, taking a , b , c to be those three tests which make $r_{ab}r_{ac}/r_{bc}$ numerically greatest, this value shall not exceed unity and shall occur with a positive sign.*

VII. THE NECESSARY AND SUFFICIENT CONDITION FOR g TO BE UNIQUE EXCEPT IN SIGN.

We have already pointed out that equations (17) and (18) both contain a term involving the *indeterminate* set of N numbers i , which are arbitrary except that their arithmetic mean is zero, their standard

¹ The possibility of r_{ag}^2 exceeding unity was first pointed out by Heywood (8), p. 497.

deviation unity, and their coefficients of correlation with each of the K linearly independent sets $a, b, \dots$ (all expressed in standard measure) are zero. It is easy to show that these conditions give K linear equations and one quadratic for $(N - 1)$ unknowns. If $(N - 1)$ were less¹ than $K + 1$ there would be no solution and the indeterminate part of g would be zero. However in practice N is taken large to reduce the error of sampling as much as possible and so is much greater than K , and i is then highly indeterminate, and consequently g is indeterminate also, unless the coefficient of i in equation (17) is zero,

$$\text{i.e. unless} \quad (1 + S)^{-\frac{1}{2}} = 0$$

$$\text{giving} \quad S = \infty \quad \text{.....(25),}$$

$$\text{i.e.} \quad \Sigma \frac{r_{ag}^2}{1 - r_{ag}^2} = \infty \quad \text{.....(26).}$$

When the tetrad relations are satisfied, formal expressions for $g, s_a, s_b, \dots$ exist. When the two conditions of section VI are satisfied, these expressions are real. When (26) is satisfied, these real expressions are unique except in sign. Thus for the two-factor theory to be valid in a form applicable to psychology, three separate sets of conditions are necessary and sufficient.

The condition (26) can be satisfied in two ways. If we can devise a set of three tests such that $r_{ab}r_{ac} = r_{bc}$, then $r_{ag}^2 = 1$, and (26) is satisfied irrespective of the second and following terms in the summation. In this case (17) reduces to $g = \pm a$, so a by itself is a test of g .

If no such test can be found, we can always satisfy (26) by taking the number of tests to be infinite. This statement has been disputed (12), on the ground that it assumes the divergence of the series S . But a series is bound to diverge if all its terms exceed a definite minimum value, which will be the case here if $r_{ag}^2, r_{bg}^2, \dots$ all exceed such a value.

But

$$r_{ag}^2 = r_{ab}r_{ac}/r_{bc}$$

$$> |r_{ab}r_{ac}|, \text{ as } |r_{bc}| < 1,$$

so the only way for r_{ag}^2 to become small is for at least one of the coefficients of correlation such as r_{ab} to become so. For convergence an infinite number of them would have to diminish indefinitely, and in practice such tests would not be applied.

We next investigate the condition that the numerical value of g should be unique to a given degree of approximation. One² method of formulating this is to take the ratio of the standard deviations of the

¹ Heywood (8), p. 489, showed that K , the number of linearly independent sets, cannot exceed $N - 1$, so the only possible value of $N - 1$ less than $K + 1$ is K .

² For another method see the end of section X.

indeterminate and determinate parts, expressed as a percentage. From (17) and (12) these two standard deviations are respectively

$$(1+S)^{-\frac{1}{2}} \quad \text{and} \quad \{S/(1+S)\}^{\frac{1}{2}},$$

so the condition that the ratio should be p per cent. is

$$S^{-\frac{1}{2}} = p/100,$$

or

$$S = 10,000/p^2 \quad \dots\dots(27).$$

Reverting to the special case where $r_{ab} = r_{ao} = \dots = r$, (27) becomes

$$Kr/(1-r) = 10,000/p^2 \quad \dots\dots(28).$$

For values of r such as occur in practice (cf. section X), equation (28) will give a very large value of K . For example, if $r = 0.5$ and $p = 1$, $K = 10,000$, a number of tests far too great to be applied. Even for the low order of accuracy given by $p = 10$, K is as great as 100. The sets of tests quoted in section X have $K = 5$ or $K = 8$, and the largest value of K ever applied, as far as I know (13)¹, is only 32. It is evident that the only practicable way to make the indeterminate part of g negligible is to find an r_{ag} which is nearly unity.

If $r_{ag} = 1 - \epsilon$, where ϵ is small,

$$\frac{r_{ag}^2}{1 - r_{ag}^2} = \frac{1 - 2\epsilon + \epsilon^2}{2\epsilon - \epsilon^2} = \frac{1}{2\epsilon} \text{ approximately.}$$

To a good approximation the value of S will be the same, if no others of $r_{ba}^2, r_{og}^2, \dots$ are near to unity, and $S^{-\frac{1}{2}} = \sqrt{(2\epsilon)}$. Equation (27) will then give

$$p^2 = 20,000\epsilon \quad \dots\dots(29).$$

Thus if $p = 1$, $\epsilon = 0.00005$ and if $p = 10$, $\epsilon = 0.005$, so to make the indeterminate part of g reasonably small we need a numerical value of r_{ag} very close indeed to unity. Such a value has been found (see section X), but it cannot be relied upon, for two reasons. In the first case the tests used do not satisfy the tetrad relations exactly, and the value of r_{ag} used is a kind of average of several values. A slight variation in the method of determining this average makes a considerable percentage difference in the value of ϵ . In the second case, the observed results are affected by *attenuation* due to experimental errors. The usual correction for attenuation certainly leaves the tetrad relations unaffected², but it affects r_{ag} and so affects the formula (17) for g . If the correction were perfectly accurate we could easily apply it to (17), but unfortunately the result cannot be guaranteed in individual cases. This will be obvious from the numerical example in section X.

¹ I have not yet been able to examine this reference.

² Spearman, *loc. cit.*, p. vi.

VIII. THE EFFECT OF POOLING TESTS.

E. B. Wilson (14), (15), (16) has raised the objection that the uniqueness of g is, at the best, only relative to the 'set-up,' and that if we are to start upon a system of weighting which leads to new scores we can, while keeping to the hierarchical systems, do pretty much what we want with g .

We proceed to examine this. Let us pool the tests a and b , replacing them by

$$a' = w_1 a + w_2 b \quad \dots\dots(30).$$

If a' is in standard measure, like $a, b, \dots$, we get, squaring and averaging,

$$\begin{aligned} 1 &= w_1^2 + 2w_1 w_2 r_{ab} + w_2^2 \\ &= w_1^2 + 2w_1 w_2 r_{ag} r_{bg} + w_2^2 \\ &= (w_1 r_{ag} + w_2 r_{bg})^2 + w_1^2 (1 - r_{ag}^2) + w_2^2 (1 - r_{bg}^2) \dots\dots(31). \end{aligned}$$

Multiplying (30) by c and averaging, we get

$$r_{a'c} = w_1 r_{ac} + w_2 r_{bc}.$$

Similarly

$$r_{a'd} = w_1 r_{ad} + w_2 r_{bd}.$$

$$\begin{aligned} \text{Therefore } r_{a'c} r_{a'd} - r_{a'c} r_{a'd} &= w_1 (r_{ac} r_{ad} - r_{ad} r_{ac}) + w_2 (r_{bc} r_{bd} - r_{bd} r_{bc}) \\ &= 0 \quad \dots\dots(32), \end{aligned}$$

so the tetrad relations are unaffected by the transformation.

$$\begin{aligned} \text{Also } r_{a'g}^2 &= r_{a'c} r_{a'd} r_{c'd} = (w_1 r_{ac} + w_2 r_{bc})(w_1 r_{ad} + w_2 r_{bd}) r_{cd} \\ &= w_1^2 r_{ac}^2 + 2w_1 w_2 r_{ac} r_{bc} r_{cd} + w_2^2 r_{bc}^2 \\ &= (w_1 r_{ac} + w_2 r_{bc})^2 \quad \dots\dots(33). \end{aligned}$$

Equation (17) suggests that g may be unaltered if we pool a and b , taking

$$w_1 = \lambda r_{ag} / (1 - r_{ag}^2) \quad \text{and} \quad w_2 = \lambda r_{bg} / (1 - r_{bg}^2) \quad \dots\dots(34).$$

The factor λ is determined by (31), which gives

$$\begin{aligned} 1 &= \lambda^2 \left[\left\{ \frac{r_{ag}^2}{1 - r_{ag}^2} + \frac{r_{bg}^2}{1 - r_{bg}^2} \right\}^2 + \left\{ \frac{r_{ag}^2}{1 - r_{ag}^2} + \frac{r_{bg}^2}{1 - r_{bg}^2} \right\} \right] \\ &= \lambda^2 (s^2 + s), \text{ say} \quad \dots\dots(35). \end{aligned}$$

(33) and (34) give

$$r_{a'g}^2 = \lambda^2 \left\{ \frac{r_{ag}^2}{1 - r_{ag}^2} + \frac{r_{bg}^2}{1 - r_{bg}^2} \right\}^2 = \lambda^2 s^2 \quad \dots\dots(36),$$

so, from (35),

$$\frac{r_{a'g}^2}{1 - r_{a'g}^2} = \frac{\lambda^2 s^2}{\lambda^2 (s^2 + s) - \lambda^2 s^2} = s \quad \dots\dots(37),$$

and $S = \Sigma \frac{r_{ag}^2}{1 - r_{ag}^2}$ is unaffected by the pooling.

Also from (37) and (36),

$$\frac{r_{a'g}}{1 - r_{a'g}^2} = \pm \frac{1}{\lambda} \quad \dots\dots(38).$$

From (38), (30) and (34),

$$\frac{r_{a'g} a'}{1 - r_{a'g}^2} = \pm \left\{ \frac{r_{ag} a}{1 - r_{ag}^2} + \frac{r_{bg} b}{1 - r_{bg}^2} \right\}.$$

Hence when we use the positive sign the pooling does not affect the formula for g at all. Similarly we can pool more than two tests, if the weights are in the ratio

$$r_{ag}/(1 - r_{ag}^2) : r_{bg}/(1 - r_{bg}^2) : r_{cg}/(1 - r_{cg}^2) : \dots$$

No other weighting will leave g unaffected.

Wilson considers the effect of replacing a and b by

$$a' = w_1 a + w_2 b,$$

$$b' = w_3 a + w_4 b,$$

and shows that in general this gives

$$r_{cg'} \neq r_{cg}.$$

But as Irwin¹ has pointed out, this transformation in general violates the tetrad relations, and in the exceptional case when these relations are unaffected, $r_{cg'} = r_{cg}$. Spearman(17) had previously made the equivalent statement that in general the new tests a' and b' *overlapped*, i.e. had in common specific factors as well as g , and so were not of the form of a and b given in equations (1) and (2), from which the tetrad relations were deduced.

To sum up, in calculating g , linear combinations of tests must in general be avoided, but the pooling of tests is allowable provided that the weights are taken in the ratio given above. The psychological significance of this prohibition of the general pooling of tests is not clear. It may indicate that the concept of a numerically unique g is artificial and so is upset by a natural rearrangement of the data, or on the other hand that the arrangement of the data is itself artificial because it combines results of different psychological nature. However this general prohibition of pooling is not operative in the important case, when the indeterminate part of g is zero either because r_{ag} is unity (provided that a is not one of the tests pooled) or the number of tests is infinite (provided that the number of tests pooled is finite). Even when these conditions are not satisfied, the effect of taking weights other than those specified above is usually small, as is shown by the experience of economists in varying the weights in weighted means.

¹ In an unpublished letter. Irwin has also constructed an example of a transformation of four variables which preserves the tetrad relations but alters g .

IX. APPLICATION TO A DICE PROBLEM.

Godfrey H. Thomson⁽¹⁸⁾ has devoted a considerable amount of time and labour to devising a dice problem in which the tetrad relations are approximately satisfied in consequence of *overlapping group factors*, and he has based on this his argument that the tetrad relations may be satisfied even when no general factor exists. His original work was complicated, but the essential points can perhaps be presented in a simple manner as follows. Take eight dice, four yellow and four violet, and reduce the sets of scores in N throws to standard measure. Denote the results by $y_1, y_2, y_3, y_4, v_1, v_2, v_3, v_4$. Consider the four sets of N numbers,

$$a = \frac{1}{2} (y_2 + y_3 + y_4 + v_1) \quad \dots\dots(39),$$

$$b = \frac{1}{2} (y_1 + y_3 + y_4 + v_2) \quad \dots\dots(40),$$

$$c = \frac{1}{2} (y_1 + y_2 + y_4 + v_3) \quad \dots\dots(41),$$

$$d = \frac{1}{2} (y_1 + y_2 + y_3 + v_4) \quad \dots\dots(42).$$

As the dice scores are independent, all the coefficients of correlation between the eight sets should be zero when N is large enough to eliminate the error of sampling. Hence, from (39) and (40), multiplying and averaging, we get

$$r_{ab} = \frac{1}{4} (0 + 0 + 1 + 1 + 0) = \frac{1}{2}.$$

Similarly, $r_{ac} = r_{bd} = \dots = \frac{1}{2}$. Hence, using equation (23), with $K = 4$

$$\begin{aligned} g &= \pm \frac{\sqrt{2}}{5} (\Sigma a) \pm \frac{i}{\sqrt{5}} \\ &= \pm \frac{1}{5\sqrt{2}} \{3(y_1 + y_2 + y_3 + y_4) + v_1 + v_2 + v_3 + v_4\} \pm \frac{i}{\sqrt{5}}. \end{aligned}$$

Thus a general factor does exist but it is not unique, and the indeterminate part is large. Although we differ from the letter of Thomson's contention that no general factor exists, we must agree with the spirit of it, for the indeterminate general factor conveys no useful idea, whereas the overlapping group factors y_1, y_2, y_3, y_4 correspond to the yellow dice common to several (but not all) of the four tests.

If we generalise this example, taking K sets of N numbers made up of $2K$ dice scores, we get

$$a = K^{-\frac{1}{2}} (y_2 + y_3 + \dots + y_K + v_1) \quad \dots\dots(43),$$

$$b = K^{-\frac{1}{2}} (y_1 + y_3 + \dots + y_K + v_2) \quad \dots\dots(44),$$

etc., where the factor $K^{-\frac{1}{2}}$ is inserted to make the standard deviations of $a, b, \dots$ unity.

From (43) and (44)

$$r_{ab} = K^{-1}(0 + 0 + K - 2 + 0) = 1 - 2K^{-1}.$$

Similarly for r_{aa} , ..., and equation (23) then gives

$$g = \pm \frac{(1 - 2K^{-1})^{\frac{1}{2}}(\Sigma a)}{2K^{-1} + K - 2} \pm \frac{(2K^{-1})^{\frac{1}{2}}i}{(2K^{-1} + K - 2)^{\frac{1}{2}}},$$

which for large values of K approximates to

$$g = \pm \frac{\Sigma a}{K} \pm \frac{i\sqrt{2}}{K},$$

of which the first term is (numerically) the arithmetic mean, and the second term is small.

Applying equation (28),

$$\frac{1}{2}(K^2 - 2K) = 10,000/p^2,$$

and if $p = 1$, $K = 142$. Thus for large values of K we have approximately a general factor, which arises from the fact that each set a , b , ... of scores depends almost entirely upon the K yellow dice, for in each set only one of these is replaced by one of the violet dice.

It may be thought that these dice problems reduce Spearman's theory to an absurdity, as it is extremely improbable that the human mind works in any way comparable to the throwing of dice. However it must be remembered that the demonstration of the existence of a real and approximately numerically unique general factor arising from mental tests is only the first step, from a psychological point of view. Having obtained what so far is merely a statistical entity, the next step is to enquire what psychological properties, if any, it possesses. Now it is claimed that g is approximately unimprovable by education or training; increases with age, at first quickly and then more slowly, at a rate independent of the individual, becoming stationary (with few exceptions) at about 16 years, and then remaining constant until affected by old age or disease; correlates very highly (0.96 according to K. J. Holzinger) with Thorndike's measure of intelligence; and, to sum up, has much the same properties as Binet's 'general innate capacity.' The specific factors, on the other hand, are claimed to be capable of great improvement by training, so the g and the s 's of mental tests have quite different properties. But the properties of dice, of whatever colour, are monotonously uniform; no one has succeeded in educating them, and they show no development with age. Thus the comparison of mental tests with dice throws breaks down at the second step.

X. APPLICATION TO PSYCHOLOGICAL DATA.

In this section it will be convenient to change the notation, using 1, 2, 3, ..., instead of $a, b, c, \dots$. It will be noticed that in the following data all the correlations such as $r_{12}, r_{13}, r_{23}, \dots$ are positive. It follows (as in section III) that $r_{1g}, r_{2g}, \dots$ are all real and all have the same sign, so, for the reason given previously, the positive sign appears to correspond to general ability. This sign only will be given in what follows.

F. G. Bonser's tests (19). For these $K = 5$ and $N = 757$ (unfortunately a heterogeneous group of 385 boys and 372 girls, of ages 8-16). The correlation table is

	(1)	(2)	(3)	(4)	(5)
(1) Mathematical judgment	—	0.485	0.400	0.397	0.295
(2) Controlled association	0.485	—	0.397	0.397	0.247
(3) Literary interpretation	0.400	0.397	—	0.335	0.275
(4) Selective judgment	0.397	0.397	0.335	—	0.195
(5) Spelling	0.295	0.247	0.275	0.195	—

From these Spearman¹ calculates $r_{1g} = 0.701, r_{2g} = 0.672, r_{3g} = 0.607, r_{4g} = 0.550, r_{5g} = 0.398$ and $S = 3.01$, giving $S^{-\frac{1}{2}} = 0.576$. The standard deviation of the indeterminate part of g is over 57 per cent. of that of the determinate part, so these tests fail to determine a g that is even approximately unique.

H. G. Stead's tests (20). For these $K = 8$ and $N = 135$ (a homogeneous group of boys of age 12+, 127 in an elementary school and 8 who had just been transferred from one to a secondary school). The correlation table is

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Completion	—	0.58	0.58	0.51	0.48	0.43	0.43	0.46
(2) Opposites	0.58	—	0.47	0.50	0.53	0.40	0.44	0.45
(3) Instructions	0.58	0.47	—	0.51	0.48	0.41	0.45	0.38
(4) Number series	0.51	0.50	0.51	—	0.34	0.50	0.35	0.40
(5) Synonyms	0.48	0.53	0.48	0.34	—	0.33	0.41	0.39
(6) Cipher	0.43	0.40	0.41	0.50	0.33	—	0.41	0.33
(7) Absurdities	0.43	0.44	0.45	0.35	0.41	0.41	—	0.31
(8) Analogies	0.46	0.45	0.38	0.40	0.39	0.33	0.31	—

These give $r_{1g} = 0.77, r_{2g} = 0.74, r_{3g} = 0.72, r_{4g} = 0.67, r_{5g} = 0.63, r_{6g} = 0.60, r_{7g} = 0.60, r_{8g} = 0.58, S = 6.8$, and $S^{-\frac{1}{2}} = 0.38$, giving an indetermination of 38 per cent., an improvement on Bonser's 57.6 per cent., but still much too large.

T. L. Kelley's tests (21). For these $K = 9$ and $N = 140$ (66 boys and 74 girls, whose ages had a mean of 12.94 and a standard deviation 1.048. All were in the same scholastic 'grade'). The tests included two of reading, two of arithmetic, and three of memory. These might be

¹ *Loc. cit.*, p. xx. My own calculations show slightly different results, but the final figures are nearly the same.

expected to violate the tetrad relations, as not being dissimilar, and this is found to be the case. However, in deciding what tests to retain we make the tetrad relations the criterion, and *not* preconceived ideas about dissimilarity of tests¹; indeed in the table below we retain three forms of memory tests. After discarding (2) reading power, (3) arithmetic speed, (6) memory for numbers, and (9) space power, we are left with the following correlation table, in which the results in brackets are corrected for attenuation.

	(1)	(4)	(5)	(7)	(8)
(1) Reading speed	—	0.0586 (0.0814)	0.1950 (0.2155)	0.2969 (0.3354)	0.1999 (0.2290)
(4) Arithmetic power	0.0586 (0.0814)	—	0.1487 (0.2099)	0.2489 (0.3592)	0.2843 (0.4159)
(5) Memory for words	0.1950 (0.2155)	0.1487 (0.2099)	—	0.6693 (0.7686)	0.4662 (0.5427)
(7) Memory for meaningful symbols	0.2969 (0.3354)	0.2489 (0.3592)	0.6693 (0.7686)	—	0.6915 (0.8230)
(8) Memory for meaningless symbols	0.1999 (0.2290)	0.2843 (0.4159)	0.4662 (0.5427)	0.6915 (0.8230)	—

The 'raw' values give $r_{1g} = 0.2800$, $r_{4g} = 0.2747$, $r_{5g} = 0.6406$, $r_{7g} = 0.9725$, $r_{8g} = 0.7625$, $S = 19.8$, and $S^{-\frac{1}{2}} = 0.225$, giving an indetermination of 22.5 per cent., as compared with Stead's 38 per cent. and Bonsér's 57.6 per cent. This improvement is almost entirely due to r_{7g} being near to unity, for, of the total 19.8 of S , 17.5 is contributed by the term $r_{7g}^2/(1 - r_{7g}^2)$. It is therefore important to consider the reliability of the value 0.9725 of r_{7g} . This was obtained from Spearman's formula²

$$r_{7g}^2 = \Sigma r_{7a} r_{7b} / (\Sigma r_{ab}),$$

where a and b take all possible (*i.e.* six) pairs of different values selected from 1, 4, 5, 8. If we compute the different values of $(r_{7a} r_{7b} / r_{ab})^{\frac{1}{2}}$ separately, we get 1.123, 1.009, 1.014, 1.058, 0.9963 and 0.7780. These are unequal because the tetrad differences are not exactly zero. Taking the arithmetic mean, we get $r_{7g} = 0.996$, $r_{7g}^2/(1 - r_{7g}^2) = 130$, and $S^{-\frac{1}{2}} = 0.09$ approximately, reducing the indetermination to 9 per cent. This good result may, however, be accidental. Of the six values used to form the mean, four are greater than unity, which means either that they indicate imaginary specific factors (see section III), or, what is more probable, that the errors in the tetrad differences are too large to give reliable results. If then at least four of these values are in error, the arithmetic mean and the final result are also unreliable.

Treating in a similar way the figures 'corrected' for attenuation,

¹ See the footnote at the beginning of section IV. In this selection we follow K. J. Holzinger (22).

² *Loc. cit.*, p. xvi.

we get the six values of r_{70} as 1.216, 1.094, 1.098, 1.147, 1.080, and 0.8431, giving an arithmetic mean of 1.080, which is greater than unity. The conclusion seems to be that the figures have been improved too much, and that the usual correction for attenuation is unreliable.

Another method of estimating the indetermination is to take the ratio of the variances, i.e. of the squares of the standard deviations, of the indeterminate and determinate parts. This is preferred by Holzinger and Spearman, and an argument in favour of it is that the sum of the variances of the two parts is a constant, namely unity. The ratio $S^{-\frac{1}{2}}$ is then replaced by $1/S$, and the percentages given above as 57.6, 38, 22.5 and 9 are replaced by the much smaller numbers 33.2, 15, 5.1 and 0.8 respectively. I hope to examine other data in the near future.

In conclusion I must express my gratitude to Prof. Spearman and my colleagues Miss A. E. M. M. Dallas and Mr F. Underwood for their valuable suggestions.

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(Manuscript received 28 December, 1932.)

THE UNIQUENESS AND EXACTNESS OF g .

By C. SPEARMAN.

It has been a great privilege to be shown the preceding paper of Prof. Piaggio in advance of its publication. By means of correspondence some misunderstandings have been easily dispelled, so that at present I have the pleasure of finding myself in almost complete agreement with him. Accordingly, the following remarks must not be regarded as in any way attempting to correct his work, but only to contribute to it a small supplement.

Most of his paper has dealt with the lack of 'uniqueness' of the ' g ' which supplies the foundation of the theory of two factors. Now, this concept of uniqueness would seem to need a little further elucidation. To take an example, the equation $x^2 = a^2$ yields the solution that $x = a$; but this result is not unique inasmuch as an equally good solution would be $x = -a$. And instead of two possible solutions to an equation, there may be a great number. Moreover, the lack of uniqueness might not be between any discrete values at all, but instead might affect a continuum. Thus in co-ordinate geometry, the position indicated by $Ax + By + Cz = 0$ is so far from being unique that it may indicate *any* point lying within a plane.

But now let us turn to indeterminateness or freedom of a different kind; that wherein there is a lack, not of uniqueness, but, more properly speaking, of *exactness*. For example, let a person be asked how far away a bridge lies; and suppose him to answer 'Four or five miles.' This is a gross case; but some degree of inexactitude dogs even the finest of measurements. Thus the rise of temperature may be measured sometimes with an error not exceeding a millionth of a degree; but vain would be the attempt to measure it without any error at all. On passing over into the realm of statistics, the inexactitude not only becomes far greater, but is more explicitly avowed. A striking example is afforded by the custom of attaching to an estimated value its 'probable error.'

What, then, is the essential difference between the two preceding paragraphs, dealing respectively with lack of uniqueness and of exactness? It would seem that in the former lack the indeterminateness or freedom is thoroughgoing; it pervades the whole affair. Whereas in the cases

charged with lack of exactness, only the measurement is indeterminate, not the magnitude to be measured. There is a unique distance from the person to the bridge (or from some point in the one to some point in the other). There is a unique value, although in general only an ideal one, around which the probable error of sampling throws a halo of indeterminateness.

From these general considerations we will pass on to the particular case at issue in the work of Piaggio; that is, the indeterminateness or freedom in the measurement of g . Now, this may occur in several different respects. One of these derives from the already mentioned error of sampling. For this error affects the estimate of the correlation of any test with g , and therefore also affects the measurement of g that is derived from this correlation (by a regression equation). Nevertheless we can and do conceive a 'true' correlation that would be yielded by an ideal infinite population. Hence in this respect (cf. sampling error), at any rate, the indeterminateness of the measurement is not properly speaking a lack of uniqueness, but only of exactness.

A second respect in which any measurement of g suffers from inexactitude is that which arises from the errors in the operation of testing. In so far as these errors are of a random nature, their effect can be gauged by means of the well-known 'reliability coefficients.' Here again, we cannot properly speak of a lack of uniqueness, but only of exactness.

Yet a third source of indeterminateness derives from the fact that the valuation of g can only be obtained from a limited number of tests, each having but a limited correlation with g . Here again, there is obviously an ideal value aimed at; it is that which would be obtained from an infinite number of tests (under otherwise suitable conditions). The case presents much analogy to that of measuring a person's reaction time. To achieve even tolerable exactness, many reactions must be measured and their average taken. For perfect exactness, the number of reactions must be infinite (besides excluding all such disturbances as fatigue); this is quite unique as an ideal, even though unattainable actually.

Finally, let us consider the particular indeterminateness of g -measurements at issue in the work of Piaggio (also of Camp). This has been denoted by the letter ' i .' Should it be regarded as a lack of uniqueness, or only as one of exactitude? In other words, have or have not the indubitably vacillating measurements an ideal measured core which does not vacillate? In reply to this question, I would say that as first discovered by Wilson and myself, no such core was manifested; the case did belong to the same class as that of multiple solutions or of location in a plane.

But at present, I would submit, this state of affairs has ceased. As was suggested by myself¹, then systematically demonstrated by Irwin², and now proved by Piaggio, this ϵ is nothing else than the error just mentioned as being due to the limited number of tests available for the purpose of measuring. Moreover, it is nothing more than the probable error given by Holzinger for the regression equation; his formula is obviously identical with the (17) of Prof. Piaggio. In short, I suggest that to charge g with not being 'unique' because of its ' ϵ ' is linguistically inappropriate and is likely to cause some serious under-valuation of its significance.

But this point at once raises another one. Though exonerating the measurement with regard to uniqueness and only impeaching it with inexactness, may not even this latter fault go so far as to render it scientifically unserviceable? That this possibly and even commonly is so, has been strongly urged by the present writer³. But the work of Piaggio would seem to paint the case in still darker colours. Now, what he writes seems to me to be unimpeachably accurate so far as it goes. But I do think that in his first method of estimating the degree of indetermination he has indicated the most unfavourable viewpoint. Far better would appear to be his second method, which is that of Prof. Holzinger. Here the natural and most significant basis for comparing the relative influences of the determinate and the indeterminate parts of the measurement of g is taken to be, not their respective standard deviations, but rather their variances. Upon adopting this improved basis of comparison, the indeterminate influence makes a startling drop; for instance, from 10 to only 1 per cent. Furthermore, I would urge that such a battery as that of Murdoch, which comprises no less than 32 tests intercorrelating by an average of nearly 0.40, affords a correlation with g amounting to nearly 0.99 and possibly—after making all due allowances—higher still. Such a measurement of g is of course incomparably superior to the very best of the current determinations of 'mental age' or of 'I.Q.' It even far surpasses many modern physical measurements; as an instance of the latter, a series of determinations of the physical fundamental magnitude ' e ' has shown a mean variation amounting to no less than 30 per cent.

We must add, however, that the uniqueness of g has been impugned on further grounds besides its ' ϵ '; notably its asserted variance for linear transformations. With these grounds we shall deal later.

¹ *Brit. J. Educ. Psych.* (1931), 1, footnote 27 on p. 21.

² *Brit. J. Psych.* (1932), **xxii**, 359 ff.

³ *The Abilities of Man* (1927), pp. xv-xvi.

THE RELIABILITY OF THE CRITERIA USED FOR ASSESSING THE VALUE OF VOCATIONAL TESTS¹.

By ERIC FARMER.

(*Industrial Health Research Board.*)

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- II. *Definition of terms* (pp. 109-10).
- III. *Objective criteria* (pp. 110-13).
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- V. *Judgments of ability* (pp. 115-19).
- VI. *Summary* (p. 119).

I. INTRODUCTION.

ONE of the main objects of research in vocational psychology is to examine the diagnostic reliability of the vocational tests themselves. Tests are given to groups of workers, or prospective workers, and if their test performance has a significantly positive association with some criterion of occupational proficiency the tests are regarded as useful. Too little attention is often paid to the validity of the criterion by which the value of the tests is judged. Obviously, unless this criterion yields a valid measure of individual differences in some defined sphere, no reliance can be placed on the results obtained from the application of the tests. In this paper I propose to discuss the criteria by which vocational tests are judged.

II. DEFINITION OF TERMS.

I shall use the term 'criterion' to designate any measure of the individual differences in occupational proficiency between the members of a group that can be compared with the measured performances of those members in psychological tests. Three principal types of criteria can be usefully differentiated one from another, but these are not necessarily exhaustive.

The first type I shall call 'an objective criterion,' meaning by it any measured performance, differentiating the individuals in a group, which

¹ A paper read to Section J at the British Association, 1932.

does not rest upon human judgment. Output or accidents are examples of objective criteria, for they record facts and not opinions and are therefore strictly objective.

The second type of criterion I shall call 'judgments of performance.' This criterion is objective because a specific task has been performed by the worker, and subjective because the value of the performance is judged by someone who is presumed to be competent to do so. Engineering trade tests are examples of judged performances; for the workers are given a task to do in a given time and the quality of their work is judged by an expert. When the criterion is of this type the work is judged, and not the workers, and in this important respect it differs from the next type I propose to define.

The third type of criterion I shall call 'judgments of ability.' An example of this type is an overseer's opinion as to the relative ability of the workers in his charge. These assessments of ability are formed from general opinions about the worker's performance in his task, his character and personal idiosyncrasies, such judgments being strongly influenced by the overseer's own character and prejudices, although they are often claimed to be judgments of performance based entirely upon observation of the actual proficiency of the worker¹. Nevertheless they are invariably judgments of a *person*, whereas judgments of performance are always judgments of *work*.

Having defined these terms, we can now proceed to discuss some of the errors that may affect each type of criterion and render it an unfit measure of the value of psychological tests.

III. OBJECTIVE CRITERIA.

An 'objective criterion' is the most satisfactory measure of the value of psychological tests if a reliable one can be obtained; but such criteria are available only in a limited number of occupations, and even when they are forthcoming they are sometimes unreliable. The chief source of error in using curves of output as criteria arises when the output is not entirely under the control of the individual who is being tested. Artificial restriction of output may result from faulty organization which causes workers to waste varying amounts of time, or from a custom that sometimes arises in groups of workers of not exceeding a certain standard of

¹ The term 'proficiency' refers merely to output, or, where this cannot be measured, to the worker's ability at his task, and does not include the various other factors such as honesty, etc., which are involved in the general conception of a good worker.

output. In either case the output curve may be an unreliable measure of individual differences.

Again, output curves are sometimes unreliable because the nature of the work or materials has changed, causing variations in output which are not directly related to the workers' capacities. Once more, it is sometimes found that output curves taken at different periods have only a low degree of correlation one with another, in which case no one would claim that they adequately represent the workers' capacities. Dr J. O. Irwin has suggested that the suitability of a set of observations as a criterion for psychological tests may be tested by 'Lexis' theory of dispersion¹. By this means we are able to test the 'disturbancy' of a set of observations and say how far the frequency distribution differs from chance. Any set of observations that yields an insignificant coefficient of disturbancy is an unfit measure of the value of psychological tests, for it does not differentiate sufficiently between individuals; the frequency distribution must differ significantly from chance. A low or imaginary co-efficient of disturbancy may arise from two causes: (1) the differences in ability are accurately measured, but although they are real, they are too small to differentiate clearly between individuals; (2) although there are large real differences in ability these differences are inadequately measured, so that the criterion is unreliable.

This suggestion of Dr Irwin's is a useful one. I have applied it to many of my criteria, one of which was found to have a significantly sub-normal dispersion. I have given a set of tests to several groups employed in this particular occupation and the results were always erratic and contradictory. It was not until I discovered that the frequency distribution of their proficiency did not differ from chance that I realized that the failure to establish any definite association between the tests and the criterion resulted possibly from the faultiness of the criterion, and not from the inadequacy of the tests.

To examine the diagnostic value of psychological tests for accident proneness, the recorded accidents of the experimental group must be used as an 'objective criterion,' but in this case special difficulties arise. The frequency distribution of accidents is skew, being of the J-shaped type;

¹ The Coefficient of Disturbancy is given by the formula

$$\frac{(\sigma^2 - \sigma_B^2)^{\frac{1}{2}}}{M},$$

where

M = observed mean,

σ = observed standard deviation,

$\sigma_B = 100p_0q_0$ (where $100p_0 = M$, and $q_0 = 1 - p_0$).

the range is usually small, and the number of accidents in any observational period is not large enough to make statistical analysis an easy matter. These characteristics of the criterion make it doubtful whether the method of correlation is the most suitable one for measuring the relation between performance in psychological tests and liability to sustain accidents. It is a method which can be used, and positive correlations with accidents will be yielded by any test that has been shown by other methods to diagnose accident proneness, but the small coefficients between psychological tests and accidents may in part be due, in this case, to the limitations imposed by the particular characteristics of the criterion. The χ^2 method obviates some of the difficulties presented in this particular instance and may more accurately measure the true relation between performance in the tests and accident proneness.

The statistical researches of Greenwood and Woods¹, and Newbold², have shown that accidents are not usually distributed by chance, but that some people are more liable to sustain them than others under equal conditions of exposure. The psychological researches that have followed have attempted to discover in what respects these accident prone individuals differ from others. The method adopted has been to compare performance in psychological tests with accident rate. In order that the recorded accidents of a group may be a valid criterion of a test purporting to measure individual differences in accident susceptibility, we must be certain that the accident frequency distribution is partly determined by individual differences in proneness and not wholly by factors affecting all members of the group equally. Miss Newbold has constructed a table which can be used to determine this matter. I have tested the accident frequency distributions of five groups of persons by this method, and in two groups they were found to be determined wholly by factors affecting all individuals alike, so that they did not measure individual differences in susceptibility. None of the psychological tests given to these two groups was found to have any significant relation with accidents by any of the statistical methods by which it was measured. In the other three groups, whose accident frequency distribution had been shown by Newbold's table to be partly determined by individual differences in susceptibility, certain tests were found to have a significant relation with accidents. If I had not tested my criteria by Newbold's method I should have been at a loss to explain why certain psychological tests had diagnostic value for accident proneness in three groups and not in two others,

¹ *Industrial Health Research Board Report*, No. 4, 1919. London: H.M. Stationery Office.

² *Industrial Health Research Board Report*, No. 34, 1926. London: H.M. Stationery Office.

although all five groups were employed in similar occupations, and I should have contributed one more contradiction to the large number that vocational psychology has already supplied.

The accidents of these two groups increased with age and experience, although the accidents of all other groups examined have been found to decrease with age and experience. It was clear, therefore, that the recorded accidents of these groups had an abnormal frequency distribution. The cause of this abnormality has now probably been discovered, but it would never have been guessed unless the reliability of the criteria had been examined.

IV. JUDGMENTS OF PERFORMANCE.

My second type of criterion—'judgments of performance'—is used mainly in the skilled trades, where the nature of the work makes it impossible to judge proficiency by output. Judgments of performance may differentiate clearly between individuals, but an examination of their frequency distributions sometimes shows that they are really only qualifying tests, *i.e.* tests in which individuals either pass or fail outright.

Two examples from my own work will help to illustrate the difference between judgments of performance that really differentiate between individuals and those that are merely qualifying tests.

The ability of several large groups of apprentices whom I have tested is judged by the individual's performance in a specific task. Marks are assigned for quality of work, and these marks show a Gaussian frequency distribution and a high coefficient of disturbancy. A further proof of the reliability of the test is that the mean and range are similar for different groups, although different examiners examined each group. These same

Table I. *Correlations between preliminary and final practical tests for skilled apprentices*¹.

Group	No.	<i>r</i>
1	524	0.463
2	259	0.706
3	347	0.498
4	333	0.566

apprentices during their period of training are given a similar but more elementary test by other examiners, and the correlation between these two independent tests is significantly positive (see Table I). Judgment of performance, in this case, therefore, appears to differentiate with some

¹ All significant coefficients in this and the following tables are in heavy type.

degree of accuracy between the ability of the individuals forming the group.

Ability in another skilled occupation was also tested by a 'judgment of performance,' but the marks ranged from 98 per cent. to 100 per cent. and there were no failures. It is clear that a criterion with such a frequency distribution does not differentiate between individuals. On examination it was found that a preliminary test had taken place some time before and only those who passed this were allowed to proceed to the final test. Since every candidate passed the final test, they were given marks varying from 98 per cent. to 100 per cent. in the hope that it would encourage competition and keep the candidates keen. It was also gratifying to the authorities to say that none of those they had fully trained scored less than 98 per cent. in the final test. Thus the only real differentiation between individuals in this instance was between those who were allowed to compete in the final test and those who were not.

The criterion for a set of tests which I was applying was the 'judged performance' of several groups of workers in four different trade tests. When the frequency distributions of these four tests were examined by Lexis' theory of dispersion they were found not to differ significantly from chance. Moreover the mean performance of some groups lay outside the range of other groups, although the marks assigned for proficiency were claimed to be strictly comparable from group to group. There was little or no correlation between the four different tests. This in itself does not necessarily render the final assessment, on the basis of the test performances, unreliable, for each test may have measured different and non-related functions, all of which were necessary for efficient performance. Nevertheless, in reliable criteria one usually finds some positive degree of relation between different trade tests purporting to measure different aspects of proficiency. One of these trade tests was then given several times to the same subjects, and its reliability, measured by inter-correlating the scores obtained each time it was given, was so low as to be entirely negligible. Yet the frequency distribution of this criterion did not appear to the eye to be unduly skewed, and so approximated to the normal shape that the criterion might easily have been regarded as reliable unless it had been examined in the way I have described.

The correlations between the psychological tests and this 'judgment of performance' were very erratic. Some groups yielded significantly positive results and others yielded insignificant, or significantly negative results. If only one group had been tested, and the criterion not properly examined, erroneous positive or negative conclusions might have been

drawn. In fact the only conclusion that can be drawn is that the diagnostic value of the psychological tests for this occupation has not yet been measured, on account of the unreliability of the criterion.

V. JUDGMENTS OF ABILITY.

'Judgment of ability' is not a very satisfactory term, for such a judgment is sometimes nothing more than a crude character assessment in which prejudice and favouritism may play a part. However, a 'judgment of ability' differs from a 'judgment of performance' in that it claims to assess the ability of an individual and not simply the quality of a specific piece of work. Although, in practice, the two kinds of criteria may overlap, 'judgments of ability' are subjective, for they record the impressions made by one individual upon another. In some cases they purport to record the impression made by an individual's work, rather than the impression made by the individual himself, but this impression is a general one and is not gained by examining the work in isolation, with all other considerations excluded.

The value of 'judgments of ability' depends on how closely they are related to the specific work involved. Often they are little more than opinions about ideal qualities which, for no apparent reason, are supposed to be involved in the occupation. The assumption underlying such opinions seems to be that an 'ideal' man in the Aristotelian sense, one who possesses a full measure of all the virtues combined with perfect poise, is the one required for the occupation. Such individuals are rare and we cannot expect to have a whole department composed of them. Moreover they will be able to find an appropriate occupation in life without the aid of vocational tests, and their place is not likely to be found in those occupations where vocational tests are usually applied.

The National Institute of Industrial Psychology has devised methods of assessing ability which bring these judgments into touch with reality. Lists of qualities like speed and accuracy, carefully defined, and definitely connected with the work, have been constructed. An examiner is asked to mark each individual on a fivefold scale for each particular quality in question, keeping all others out of consideration. The marking should be made to conform to a normal frequency distribution, any departure from which should be a matter of special inquiry. If this is not done the measure will be biased by any tendency of the judges to mark unduly low or unduly high. Difficulties may arise because it is possible for one judge to assess a small number of persons only; but when several small groups are combined the frequency distribution should be normal for the combined

group if the assessment has been carried out properly. In any case small groups are inadequate for statistical purposes and unless it is possible to get a final group of sufficient size to yield a normal frequency distribution the criterion cannot be used to measure the value of psychological tests.

I believe it is along these lines that 'judgments of ability' can be improved and made to yield reliable criteria. In some cases they may be changed by this method from 'judgments of ability' to 'judgments of performance' and so gain in objectivity.

Table II. *Coefficients of contingency between four supervisors' judgments of ability.*

93 subjects.		
Between supervisor I and II	C_1	0.506
" I and III		0.674
" I and IV		0.528
" II and III		0.636
" II and IV		0.621
" III and IV		0.569

'Judgments of ability' of the same individuals made by different assessors are sometimes compared and if they are positively related they may be assumed to be reliable criteria. Such an assumption is not always justified. The assessments are sometimes not made independently but are the result of consultations, deliberate, or otherwise, between the persons making the judgments. In this case we are not dealing with independent judgments; all we know is that two or more people discussing the abilities of a group of individuals have come roughly to the same conclusion.

Sometimes 'judgments of ability' are made by ranking the individuals in a group, for purposes of correlation. I think it impossible, however, for any assessor, using this method, to rank individuals for their ability sufficiently accurately and in a sufficiently large group, to warrant statistical analysis. It may be possible to get fairly accurate 'judgments of ability' by means of fivefold categories, provided proper instruction is given and a strict adherence to the normal frequency distribution is maintained. Most overseers are able to pick out the 'really good' and the 'really bad' workers, leaving the majority of the group in the intermediate category. 'Judgments of ability' in this form I believe to be more reliable than any attempt at ranking, and also better than the fivefold method, except when special instructions are given and carried out. The fivefold category method may be the most reliable when we can be sure that all the necessary precautions have been observed.

It may be questioned whether either 'judgments of performance' or of 'ability' really measure accurately the qualities necessary for efficient

performance in an occupation. The ideal measure of occupational proficiency is an 'objective criterion'; both other types of criteria are only makeshifts.

I have been able to compare a 'judgment of ability' with an 'objective criterion' in one occupation, and a 'judgment of ability' with a 'judgment of performance' in another (see Table III). There is no significant agreement between the 'subjective' and 'objective' criteria. There is, however, a very close and highly significant agreement between the 'judgment of ability' and the 'judgment of performance.' No conclusions can be drawn from these isolated instances, but it is interesting to note that the 'judgment of ability' which showed no agreement with the 'objective criterion' in the same occupation was made by a foreman who divided his workers into five categories, according to his opinion of their proficiency; whereas the 'judgment of ability' which had a significant relation to the 'judgment of performance' was given in the form of a short description of the worker's character. In these character assessments it was stated whether the worker was keen, clever but lazy, stupid but industrious, and so forth. When the workers were divided into two groups by independent assessors, there was a very close and significant association between these and the two groups into which they were divided by a trade test, carried out by independent examiners. I think this method of judging ability may be recommended, as being well within the capacity of most foremen.

Table III. *Contingency between foreman's estimate and objective criterion among unskilled workers.*

96 subjects.

$C_s = 0.355$ (insignificant).

Fourfold correlation between judgment of performance and judgment of ability by independent assessors. Skilled workers.

16 subjects.

$Q_s = 0.922$.

Another method of testing the reliability of 'judgments of performance' and of 'ability' is to compare them with the score in an entrance examination. In all the instances in which I have been able to compare 'judgments of performance' with an entrance examination a significantly positive correlation has been yielded. In no case in which I have compared a 'judgment of ability' with an entrance examination have I found any positive agreement between the two (see Table IV).

Table IV. *Correlation between entrance examination and final trade test (judgment of performance) for skilled workers.*

Group		
A	0.410	524 subjects
B	0.312	259 "
C	0.516	347 "

Correlations between an intelligence test and final trade test for skilled workers.

Group		
A	0.288	524 subjects
B	0.289	259 "
C	0.336	347 "

Correlations between entrance examination and supervisors' judgment of ability.

(Numbers of subjects not available.)

Group		Group	
A	0.11	F	-0.0015
B	0.12	G	0.00
C	0.429	H	0.018
D	0.22	J	0.08
E	-0.03		

In the skilled trades, entrance to which is earned by examination, intelligence is necessary for subsequent proficiency. Intelligence is also one of the qualities measured by an examination, so that for this reason we should expect a correlation between examination order and a valid measure of proficiency. In all the skilled trades I have tested I have found a significant correlation between entrance examination, intelligence measured by various intelligence tests and criteria that have been shown independently to be reliable (see Table IV). If therefore some criterion which cannot be shown by other means to be reliable fails to correlate with the entrance examination in a skilled occupation, it should be regarded not as evidence that the examination has no selective value, but that the criterion has little reliability.

When no correlation is found between an entrance examination and a criterion, it is sometimes taken as an indication that the entrance examination is of low diagnostic value for the qualities involved in the occupation; but this conclusion does not necessarily follow. In the first place, since the subjects have been selected according to their score in the examination, we should not expect a high correlation between the examination scores of those accepted and their subsequent occupational

proficiency, because those accepted lie in the upper part of the frequency distribution and therefore are not representative of the whole group of applicants. This selection will, therefore, lower to an unknown extent the correlation between the entrance examination and any criterion of proficiency.

VI. SUMMARY.

The reliability of psychological tests for vocational purposes is measured by comparing performance in the tests with some measure of industrial proficiency. Unless this measure is itself a reliable indication of the worker's industrial ability it cannot be regarded as a proper criterion for determining the reliability of psychological tests for vocational purposes.

Three types of criteria are distinguished in this paper.

(1) 'Objective criteria' are records of industrial proficiency in which human judgment does not enter, *e.g.* output.

(2) 'Judgments of performance' are judgments of work by competent examiners without knowledge of who did the work.

(3) 'Judgments of ability' are judgments of the proficiency of individuals who have been observed at work.

Some of the errors that may enter into each of these criteria are pointed out and suggestions are made for testing the reliability of criteria. The paper is preliminary in nature, and no definite conclusions are drawn. Its object is merely to call attention to one of the sources of error in vocational psychology and to suggest certain methods of overcoming it.

(Manuscript received 26 September, 1932.)

PUBLICATIONS RECENTLY RECEIVED.

The Psychology of a Primitive People. By S. D. PORTEUS. London: Edward Arnold & Co. 1931. Pp. xv + 438. 30s. net.

No psychologist whose interests carry him outside of the laboratory and into the everyday life of human beings, can afford to neglect this book. It is the first piece of comprehensive field work carried out by a psychologist in a realm usually cultivated only by the ethnologist, the social anthropologist or the missionary. Whatever criticisms may be advanced concerning the methods used by Prof. Porteus, the book will stand as a pioneer record, important in itself and opening up many fascinating lines for future research.

Dr Porteus carried out his work in North-west and Central Australia. He rightly concluded that "to judge fairly of the intelligence" of the primitive people with whom he was dealing he must "consider the evidences of their adaptability to their own peculiar environment," and study "the psychological bases of their social customs, organization and habits of life." The first part of his book, then, draws a picture, vastly interesting in its detail, of the environment, both physical and social, of the natives he studied. Although a great deal of this ground is exactly the ground traversed by the social anthropologist, it is here studied and interpreted largely in psychological terms. The second part of the book deals with "the underlying psychology of Australian social organization, totemism, ceremonial observances and initiation," and also, in the last six chapters with physical measurements, psycho-physical tests, tests of temperament and intelligence, memory and specially devised tests of the intelligence of the aboriginal child. It will be seen that this final part is an attempt to carry experimental psychological method into field investigation. Most psychologists will feel a little disappointed that Prof. Porteus, in his experimental examinations, did not break away more radically from forms of test which have been developed and studied almost wholly in communities of a high social level. He used only two sets of tests specially devised to "accord with the cultural background of the aborigines": a foot-prints test and a dots recognition test. Even these were presented in a form that appears rather highly sophisticated. His conclusions that the tests showed his native observers to be less inefficient than might have been expected, but inadaptable to civilized life, are not very helpful. One cannot help feeling that the very thorough and valuable study of social customs and outlook which preceded the use of specific tests, might have led to an attempt to develop an experimental technique less tied up with methods specifically developed in a very widely different cultural background. The results of this might not have had the appearance of being comparable in a quantitative sense with those obtained elsewhere, but they would certainly have been comparable in other ways and they might perhaps have started a technique for field experimentation different from, but equally as valuable as, that of the laboratory type of study.

However, nobody who has attempted anything like it will be inclined to minimize the difficulties of this kind of work. No criticism can detract seriously from the value of this important and very attractively presented piece of research. It is greatly to be hoped that Dr Porteus's book will have many successors all on as high a plane of achievement as his. For in this way alone will social psychology eventually be established upon a sound basis.

Remembering: A Study in Experimental and Social Psychology. By F. C. BARTLETT. Cambridge: University Press. 1932. Pp. x + 317. 16s. net.

This important addition to the Cambridge Psychological Library presents a closely reasoned statement of a profound theory of memory which demands most careful consideration by psychologists. Being based entirely upon the author's own researches,

the book is not a complete treatment of the subject, but an original contribution to it. Its influence will extend beyond the ostensible limits which Prof. Bartlett has set himself, for it reflects and defends an attitude towards the whole science, and the fundamental ideas upon which the theory is based may be found capable of important applications in other departments of psychology. He adopts the functional outlook consistently, and gives us a new respect for purely qualitative methods. Memory is viewed as a mode of response integrally bound up with the whole fabric of mental activities. With admirable thoroughness he examines the perceptual experience which supplies the material remembered, and the social setting which conditions the individual who learns and remembers.

The development of memory processes is taken as parallel to that of the distance receptors. The function of memory is to facilitate efficient responses by releasing the organism from bondage to present circumstances. Head's theory that schemata of postural reactions are formed which control new reactions, is generalized (with critical reserve) to cover the more complex case of ideational response. The term 'schemata' is borrowed with some misgivings and used to denote psychological material as organized. The development of consciousness enables the organism to avoid the primitive tendency to exact serial repetition by means of reconstruction, in which images and language play a decisive part. In remembering the organism is called upon to respond by recall of material, but the demand is always particularized, and never (in typical instances) a mere re-living of an experience. The process is therefore constructive, and not to be sharply distinguished from imagination and thought. The experimental data suggest that with the lapse of time the formal character of the organization becomes relatively more important. But the experiments deal only with cases in which explicit recall of material is demanded, so that the individual has to *react* to his schemata. Summarized thus the theory may seem too abstract to be serviceable, but a study of the very full discussion will show that it must be taken seriously. If Prof. Bartlett's conclusions are accepted, much more than the traditional theories of memory will need to be re-focused. The difficulty of the argument, and it is not inconsiderable, is lessened by the extremely careful arrangement and statement, but the book is essentially one for close and leisurely study. Its wealth of detailed statement cannot be discussed in a short review, nor can we discuss the extensive series of experiments.

Since remembering is continuous with immediate experience, and both are shaped by the constitution of the individual, which in turn depends upon social relationships, the author finds it necessary to attempt an independent analysis of those relationships. This section of the book may well be studied by itself as an introduction to Social Psychology. We have at last a discussion which avoids wide and picturesque generalities in favour of the careful and minute study of facts. We commend these all too few chapters as a model of how social psychology should be approached.

The Psychology of Pleasantness and Unpleasantness. By J. B. BEEBE-CENTER.
New York: D. Van Nostrand Company. 1932. Pp. viii + 427.

This very careful and well-written essay should very easily hold the field in its most difficult subject for a considerable time to come. Dr Beebe-Center was very justly impressed by the fact that "the psychology of pleasantness and unpleasantness is at present a potential body of knowledge rather than an actual one," and he set himself to provide a remedy by bringing the facts together "into a single orderly structure." He has realized his aim very excellently and completely, and he has done more than this, for he has produced also a readable and often a stimulating book. There is pretty well no question about the psychology of affect that has been raised which is not here lucidly touched upon, with all the more important experimental detail described, and adequate references for further study. Not only are the essential facts given, but the theories also are considered in a final chapter. Here as elsewhere, Dr Beebe-Center achieves a very high level of concise accuracy. His own predilection is for a type of physiological theory connected with the views of Köhler and of others who adopt more or less the same psychological point of view. He is greatly impressed by the high correlation observed by Nage and Hunt between hedonic tone and organic

pressure, and he thinks it probable that "hedonic-tone depends upon a specific type of process in sense-organs, namely that which under sensory instructions or their equivalent, mediates bright and dull pressure." He believes that affect depends either directly or indirectly upon some character of the process which he calls 'density.' The definition of 'density' as probably a special form of 'stimulus intensity' or possibly of frequency, does not make a dim matter much clearer. But too much must not be made of this. At the most it is only put forward as a speculative hypothesis nearly every point of which must be held subject to experimental revision. The book is throughout a good and fair exposition, and it contains many suggestions which should lead to new and controlled experimental investigation.

The Science of Psychology. By RAYMOND H. WHEELER. London: Jarrolds. 1931. Pp. xi + 556. 21s. net.

Prof. Wheeler here presents a new text-book of psychology designed to cover the whole subject and to be suitable both for the student who is beginning his study of psychology and for those who have already some acquaintance with the subject. The general point of view adopted is that it is of little use to present masses of facts apart from principles which show how the facts may be related. The principles with which Prof. Wheeler finds himself in chief sympathy are those which have been expounded by Wertheimer, Köhler, Koffka and others of the *Gestalt* school. In arrangement the new text-book is novel. Psychology is held to deal with conscious behaviour and its conditions. Both the behaviour and its conditions have wide ramifications and it is best to begin to study them in their customary setting, that of the social group. Accordingly the book begins by a study of social behaviour, and proceeds to take up in turn special aspects of this, i.e. intelligent behaviour, emotive behaviour, learning problems, and only after a consideration of all these discusses the more conventional topics of the psychological text-book: reactions, attention, space perception, the reactions of the special senses. The last topics of all to be dealt with concern the nervous system in relation to behaviour. There is much to be said in favour of this novel arrangement. It brings a realism and human interest into the study of psychology which are very often lacking in the more usual method of approach through relatively simple laboratory exercises. For the student who approaches psychology with a philosophic background or even with an equipment of general theoretical biology probably no better road could be made. But most teachers may be inclined to agree that there is no one royal way to success in the understanding of psychology. A very large class of modern students, for example, come to psychology with predominant physiological and medical interests. These are likely to find the early parts of Prof. Wheeler's book both difficult and rather airy. But a judicious teacher will be able to use this book in any order that he pleases, and in that way it should certainly come to occupy an important place among the many modern introductions to the science of psychology. It covers a vast amount of ground; it gives plenty of suggestions for additional reading; it is alive and interesting throughout and it does not leave the student, as too many texts do, wondering what that great accumulation of facts, which are said to have psychological bearings, lead to or indicate. Moreover, and perhaps most important of all, it will present to any intelligent student many problems which would repay his investigation, and give to many the sort of orientation towards these problems that is likely to set them in the way of profitable investigation.

Character in Human Relations. By HUGH HARTSHORNE. New York and London: Charles Scribner's, Sons. 1932. Pp. xiv + 367. 10s. 6d. net.

One way or another nearly all the recent work on character as an achievement within a social group, is dealt with in this book. First Dr Hartshorne describes the present situation, indicating the methods which are proposed for the formation of character and varied applications of those methods in school, in town, in club and in state schemes. A reader in this country can here get an interesting introduction to the numerous experiments which are being made in America, and many references are

given so that he can pursue his study further if he wishes. The second section deals with typical theories of character. In the third part Dr Hartshorne presents at considerable length his own picture of "character as effective functioning." Much of this, particularly those parts that deal with the specific function of the social group in character formation, is worthy of close thought and study. Finally, an attempt is made to show in what ways groups, and in particular schools, may be organized with a view to character training.

The book should not be missed by any social psychologist. It could have been much better if it had been more severely pruned. But it presents a mass of information which it would be most difficult to obtain elsewhere, and it contains many stimulating ideas.

Alcohol and Man. Edited by HAVEN EMERSON. New York: The MacMillan Company. 1932. Pp. xi + 451. 18s. net.

Here are a series of essays by eminent American physiologists, pharmacologists, anatomists, pathologists, physicians, psychiatrists and a psychologist, dealing with the effects of alcohol on man in health and disease. The whole volume contains six parts dealing with: the effects of alcohol on human functions; on the cell and in heredity; as a poison and a medicine; in relation to body resistance and pathology; on conduct and mentality; and on longevity, mortality and morbidity. Throughout, the book is written with restraint, with abundance of evidence and in a very clear manner. There is no exaggeration and there is on the whole a great amount of agreement. Dr Walter R. Miles, who writes on the effects of alcohol on man's conduct and mentality, has produced just the delightful and accurate essay that would be expected. No doubt enthusiasts of every school of thought will find things in this volume to back up their own point of view. The facts are presented, the interpretations are moderate. On the whole on most counts the case goes against anything but the most exceedingly careful and moderate use of alcohol. The book claims careful study in many directions, and deserves what it claims.

Psychologie Pathologique du Suicide. Par F. ACHILLE-DELMAS. Paris: Félix Alcan. 1932. Pp. xi + 237. 30 fr.

This book is both a very whole-hearted polemic against Prof. Maurice Halbwachs' recent book, *Le Suicide*, and an original contribution to its topic. Dr Achille-Delmas is convinced that so far from suicide being exclusively determined by social causes it is not even primarily so. Thus the Durkheim school, of which Prof. Halbwachs is a distinguished adherent, is radically on the wrong lines in its attitude towards suicide. The first part of this book consists of an extremely interesting and devastating attack upon statistical methods of argument. The second part is an equally interesting study of suicide as a psychopathological form of behaviour having a predominantly individual determination. Suicide is defined as "*l'acte par lequel se donne la mort tout homme lucide qui, pouvant choisir de vivre, choisit cependant de mourir, en dehors de toute obligation éthique.*" In this sense suicide is most frequently the expression of a cyclothymic personality with strong pathological predisposition towards melancholia, and less frequently due to a "hyperemotional" temperament. It is urged that at the most amelioration of social conditions might be expected to eliminate about 15 per cent. of suicides; the rest are an affair of "*un mécanisme biopsychologique, où il n'entre rien de social.*" An original, striking and learned chapter deals with *La légende du suicide philosophique*. The whole book forms an outspoken and able argument, very clear and downright. The psychiatric analysis might, however, be more thorough.

L'Envie: son rôle Social. Par EUGÈNE RAIGA. Paris: Félix Alcan. 1932. Pp. xv + 268. 15 fr.

Envy is a natural and inevitable reaction of the vanquished against the victors. It is different from jealousy, which at bottom is a kind of fear; and from admiration, which springs from the same sort of situations except that the persons concerned are

not competitors. Its various forms are discussed and its exhibitions in love, friendship, the family, small and large social groups, among different professional organizations, and in political, international and religious affairs are depicted with great skill. Since the tendencies which are expressed in envy are a part of our common human endowment it is futile to attempt to check them by intelligent moral observations. Rather we must observe its functions in society, and we may be able to regulate them by building up envy with other tendencies as fundamental as itself but different in direction. Then envy may play a part in developing modesty, in stimulating social effort, even in leading to a kind of natural comparison and the growth of types of social arbitration based upon a claim for justice. It will be obvious that this is precisely the kind of book that many French psychologists seem to be able to write particularly well. And it is a very good book of its kind: clear, witty, incisive and full of acute psychological observations.

Social Laws. A Study of the Validity of Sociological Generalisations. By K. D. HAR. University of North Carolina Press. 1930. (London: Humphrey Milford.) Pp. xi + 256. 18s. net.

The author passes in review a great many attempts which have been made to establish "social laws." The formulations produced are classified as (1) methodological presuppositions; (2) teleological laws; (3) statistical laws; (4) near-causal laws, and (5) dialectical laws. Of them all Dr Har is very properly and reasonably critical. Only two sets of proposed social laws, he thinks, *v.e.* Gresham's law and the Malthusian laws of population, possess "a reasonable degree of probability. If sociology is to be made an exact science the most fruitful line of approach is that of causal psychology, but at present the social sciences are merely a mass of empirical generalisations. As such, however, they provide working principles for the social arts, six of which are discussed by the author in a general and somewhat abstract manner.

The book is clever, critical and well written. It may clear the ground for an attack upon its tangled subject which may have better chances of success than those made hitherto. The English price of the volume seems excessive.

History, Psychology and Culture. By A. GOLDENWEISER. London: Kegan Paul, Trench, Trubner & Co. 1933. Pp. xii + 475 + xii. 18s. net.

Discriminating students of human culture have learned to expect with confidence that when Prof. Goldenweiser publishes an article or a book they are likely to be presented with a careful, informed, critical, suggestive and constructive study. Much of this particular book has been published before, but very few readers indeed in this country can have had access to many of the most important communications that it contains and the volume deserves a warm welcome and a wide reading. Prof. Goldenweiser deals first with various methods of approach to the study of human culture and has much to say, some of it critical but all of it sympathetic and sensible, about psychological study in this field. He then considers theories of primitive mind and culture, dealing especially with English evolutionist views, and with those of Sir James Frazer, Lévy Bruhl, Wilhelm Wundt, and Sigmund Freud. More specialized studies of totemism, religion and race follow. The volume concludes with a few essays of a slighter kind on various topics. The general approach all through is historical. It is a book that certainly ought to be in every general psychological library, a book that constantly challenges thought, one that no reader will treat lightly even when he is inclined to differ from its point of view.

World Chaos: The Responsibility of Science. By WILLIAM McDUGALL. London: Kegan Paul, Trench, Trubner & Co. 1931. Pp. vi + 119. 3s. 6d. net.

This little book deserves a wide reading and serious consideration. It should be studied not only, perhaps not primarily, by psychologists, but by all who are interested in the developments of modern civilization, and especially by those who are concerned

with scientific developments. Prof. McDougall considers that our civilization is both top-heavy and lop-sided. Not only have material inventions proceeded so fast that the normal human capacity for satisfactory adaptation tends to be outstripped, but they have been altogether predominantly along the line of physical science. For various reasons the biological and especially the social sciences have lagged far behind the main advance columns. The result is disastrous already and carries a promise of yet more serious trouble. Magnificent discoveries, McDougall thinks, demand magnificent men if they are to be supported. But we know and care very little about the production of magnificent men, or of societies based upon sure foundations. Not only so, but when mechanistic views are found to work triumphantly in one field they are blindly applied to all. The part of this lecture that is perhaps designed to make one's flesh creep is splendid. It is sweepingly controversial, and to many readers it will appear to have all the exaggeration that controversial writing usually possesses, but it is expressed with a vigour and also with a sincerity that will stir the most sluggish reader to enthusiastic adherence or condemnation. The constructive part of the lecture is, however, nothing like so satisfactory. Perhaps within the limitations of its space it could not be, but in fact not very much is said except a reiterated exhortation that social and biological science should be more seriously cultivated. As McDougall thinks that it takes fifteen or twenty years to produce a decent psychologist, the proposed remedy is a very slow one. But that too is frankly recognized.

Crime for Profit. Edited by ERNEST D. MACDOUGALL. Boston: The Stratton Company. 1933. Pp. xx + 355. \$2.00.

There is in America a National Institute on Mercenary Crime and Dr E. D. MacDougall is its president. This is one of its publications and in spite of a lot of bad and highly emotionalized writing it contains many facts and views which the psychological student of contemporary society will study with entertainment and profit. It is concerned in the main with the social disasters threatened and achieved by efforts to manipulate markets of all kinds so as to get rich quickly. The editor himself does not measure his words when he writes of these. Violent fluctuations are required by gambling operations, and gambling operations precipitate violent fluctuations: "There we have the vicious circle, strangling our beautiful, bountiful country at the throat, which vicious circle, by the eternal gods, must be broken pretty soon." It would be very unfair, however, to suggest that all the writing is in this strain. Among the many collaborators, who are drawn from the most varied professions, are a good number who write with knowledge and without exaggeration. And, for once in a way, some of the most interesting remarks concern the part that education may play in counter-acting the spread of 'racketeering' methods in industry and finance. An English reader may be considerably helped to understand a very prevalent American attitude towards War Debts if he reads Dr Baine's essay on *Mercenary Crime and International Relations*.

Occupational Misfits: A Comparative Study of North London Boys Employed and Unemployed. By SHEILA BEVINGTON. London: George Allen and Unwin, Ltd. 1933. Pp. 102. 6s. net.

This book describes research which was undertaken to attempt to answer the following questions: "Do the joint efforts of parents, teachers and officials usually direct the London working lad towards the occupation for which he is best suited?" and "Can the steady employment of some, the frequent unemployment of other lads, be ascribed mainly to factors of an economic, social or psychological character?" The methods adopted are very clearly described and consist of a careful study of home circumstances, school records and industrial experience among two comparable groups of lads, the one group of regularly employed subjects and the other of unemployed subjects. Both groups came from the same area, Tottenham in North London. The main general conclusions are that temperament and character are the most important factors determining industrial success; that present sources of vocational advice—mostly headmasters and parents—are seriously inefficient in this respect; and that any

boy who leaves school with "an occupationally aimless outlook" is seriously handicapped in his subsequent life. The main remedies suggested are a more serious attempt to equip parents to give reasonable vocational advice, and the extension of the vocational functions of schools by the definite appointment of careers masters. The experiment, which was initiated by the National Institute of Industrial Psychology, is a very important contribution to social psychology. Its aims, methods and results are described by Dr Bevington with a clarity and conciseness that offer a model of how such things ought to be done, and the book should not only be widely read but should serve to stimulate much similarly directed research.

Industrial Psychology. By MORRIS S. VITELES. London: Jonathan Cape. 1933. Pp. xviii + 652. 21s. net.

This book possesses what the felicitous Kai Lung might describe as a "breath removing" completeness. There is hardly a problem of industrial psychology, from the most general to the most specific, that is not discussed, and the whole forms by far the most extensive text-book on the subject that has yet appeared in the English language. The framework of it is simple enough. Beginning by a study of the growth of the subject it goes on to discuss vocational work, training work, safety, accidents, fatigue, monotony, motives, social problems and management. The author has a vast, and yet a well organized knowledge of his subject. He writes indeed as if time and words are cheap and can be spent lavishly, and has a liking for the detailed description and the detailed anecdote. The book is a good one, however, and any student who reads it will have a sound knowledge of what industrial psychology has attempted, what it has done, and what it may reasonably hope yet to do.

The Psychology of Sex: A Manual for Students. By HAVELOCK ELLIS. London: Heinemann. 1933. Pp. xii + 322. 12s. 6d.

The need for a small, straightforward and sane book on this subject was very great. It exists no longer, for this volume supplies the need. Not that it is or pretends to be a final discussion of all the difficulties that beset this subject, but that it does give precisely the sort of information that many people have suffered greatly from not being able to get in the past, and gives it in a manner entirely free from exaggeration and cheap emotionality. The actual topics dealt with are the following: the Biology of Sex; the Sexual Impulse in Youth; Sexual Deviation; the Erotic Symbolisms; Homosexuality; Marriage, and the Art of Love. The book is not merely a survey of the work of others; nor is it merely an account of specific original investigations. It is both, and it is a contribution to social and to individual psychology of very great importance. Throughout, its primary interest is in the normal manifestations of sex. The book is one that can be recommended strongly. Its publication should do a vast amount of good, and apart altogether from that it is a psychological study of great forcefulness and of permanent value.

Love and the Sex Emotions. By WILLIAM J. FIELDING. London: Noel Douglas. 1933. Pp. xiv + 357. 7s. 6d. net.

On the whole this is a very good book indeed. It does not appear to be the outcome of original observation, but the author has read widely and has a gift of clear exposition and a capacity to select interesting and important material. It is in places rather incautious and inclined to dogmatize about matters that are hardly yet finally settled. These tendencies are perhaps most marked in the discussion of the glandular basis of emotions, the sexual origin of the neuroses, sex and crime, sex and religion, and sexual symbolism. But in amongst all these are valuable and straightforward discussions of sex emotions, of married life, of some of the more common sex deviations and of the individual history of sex impulses. The important psychological side of the whole matter receives constant attention, and is in general dealt with well. In particular many people married or about to be married would gain help and understanding by a study of this book.

Adam et Eve ou le question des sexes. Par W. BOVEN. Neuchatel: Delachaux et Niestlé. 1933. Pp. 144. 2.50 fr.

Adam and Eve are regarded as the ideal healthy man and woman, and in this book Dr Boven sets himself to write their theoretical history from infancy to age. He is concerned with their relationships as typical, but rather exalted, members of the different sexes. On the whole the book is a good one, and eminently sensible, though it tends to become rather sentimental in many places. It is on the side of the conventions, but does not lack interest on that account, for the author finds a psychological justification for all his good advice and for all his arguments in favour of a regular and controlled sex life. It is written throughout in general terms, and is not quite so satisfactory in its way of dealing with sex deviations as in the way it discusses normal sex relationships. Dr Boven is not very impressed with the value of Freudian psychology and some of his opinions and criticisms are of value. Taken as a whole the tone of the book is exhortatory rather than informative, but there is undoubtedly more to be said for this method of approach than a good many modern writers appear to think, and in any case if a reader thinks that he must seek more detail at various points there is now no lack of books to satisfy him. From a psychological point of view the book might have gained if it had been made more concrete, but it remains a presentation of the subject that should be of value to normally endowed individuals.

Motives of Conduct in Children. By ALICE HUTCHISON. London: Jarrolds. 1933. Pp. 185. 6s. net.

This book succeeds in doing exactly what it sets out to do, and in doing it well. Addressed in the main to parents, it contains no unnecessary technicalities. It deals with problems which every parent who takes any interest in his or her children must face, and discusses in a perfectly sane and balanced manner both their significance and their treatment. The topics dealt with are the following: stealing, untruthfulness, sex instruction, indulgence in sensation, sexual precocity, backwardness, asthma, stammering, habit spasm, bed-wetting, thumb-sucking, nail-biting, fits and night terrors. The most detailed discussions deal with stealing, untruthfulness and sex, but every topic touched upon is illuminated in a helpful manner. Although the book contains little that is new to the psychologist, the latter could learn much from its clear and informed statements. To parents, nurses and the general reader, Dr Hutchison's book may be recommended strongly and without reserve. A foreword is contributed by Prof. Cyril Burt.

Jocasta's Crime. By LORD RAGLAN. London: Methuen & Co. 1933. Pp. ix + 215. 6s. net.

In this very downright and determined book, Lord Raglan is concerned with the ancient question of the origin of incest prohibitions. Psychology and anthropology jostle one another considerably throughout the argument, but the former has, on the whole, rather a bad time. For eighty-seven pages Lord Raglan is mainly critical. He considers with admirable brevity sixteen different theories and rejects them all. He then builds up his own view which is that incest prohibitions find their explanation in a number of compromises between "a very ancient magical belief that it is dangerous to have intercourse with a woman who lives on the same side of the stream, and a newer, but still ancient religious belief that in order to ensure his survival after death a man must marry his sister." The hypothesis is not regarded as established but Lord Raglan's historical studies lead him to think that it is at least more worthy of investigation than any other. Many psychologists may feel not disinclined to agree with Lord Raglan's strictures upon existing psychological theories, but they are likely to wish to know more than he is inclined to tell them about how these ancient beliefs can have obtained the importance and power that they appear to possess in human society.

Motives and Mechanisms of the Mind. By E. GRAHAM HOWE. London: The Lancet, Ltd. 1931. 10s. 6d. net.

This is a reprint of twelve lectures which were given as a post-graduate course by Dr Howe in the autumn of 1930 under the auspices of the Tavistock Square Clinic, and which were later published in the *Lancet*.

The author gives his own views on the motives and mechanisms of the mind. He has not troubled about the teachings of Freud or Jung or Adler as such, but has taken from each whatever fits into his own scheme of things. He begins with a consideration of the primary motives, Love, Protection and Power, and of the way the normal mind works, and proceeds to the evolution of mind and to the various effects of the father-*imago*, of the mother-*imago* and of the "feeling about Self" which we are assumed to inherit as the result of racial experience. A brief account of therapy is given in the last chapter. The whole is interesting and readable and is well illustrated by descriptions of clinical cases and by diagrams, which latter may add to the clarity of the text for some readers. One feels that the value of the work might have been increased if alternative views had been presented and if reasons for divergence from other authorities had been given.

On the Making of Use Acquirements; the Neuro-Psychic and other Responses. By C. J. BOND. William Withering Lectures, 1932. *On Certain Aspects of Biology.* London: H. K. Lewis, Ltd. 1932. Pp. 44.

This is a lecture which was delivered in the University of Birmingham Faculty of Medicine. The thesis which Dr Bond is defending is "that the method by which the individual cell, or the group of cells, which make up the individual organism, become adapted to environmental change is essentially the same kind of process as that by which organisms and species become adapted to changing conditions of life during racial evolution." A 'Use acquirement' is defined as "that change in the structure and function of a cell which is brought about by *direct* response to Environmental influence." Dr Bond first studies in some detail the "neuro-psychic" response, then considers muscular hypertrophy, next the response made by epithelial cells to environmental change, cancer regarded as an adaptive response, the 'use-acquirements' made by unicellular organisms, the response made by bone cells, and finally the acquired immunity reactions. It will be evident that the lecture ranges over a very wide field, but it raises some theoretical issues of the greatest importance and interest, and should serve both to direct and to stimulate thinking about the course and determination of biological evolution.

An Experimental Investigation of Eye Movements in Learning to Spell Words. By LUTHER C. GILBERT. Psychological Monographs, XLIII, No. 3 (Whole No. 196). Psychological Review Publications. 1932.

This monograph describes how the eye movements made by children and adults in studying the spelling of difficult words were photographed upon a moving film, by the method of Dodge, C. T. Gray and others. From the thorough and careful records obtained, it was found that good spellers could be differentiated from poor spellers by the smaller number and duration of their fixation pauses, and the smaller number of regressions, especially those of the wildly oscillating kind. As spelling became perfected, the study of difficult words proceeded more speedily, regularly and with increasing width of perceptual span. Some letter combinations became familiar before others, and the easy sections were then rapidly dismissed, while the harder sections were carefully analysed. Poor spellers failed to develop this procedure; and the author concludes that it should be demonstrated in teaching spelling. But nothing is said of the importance of connecting the spelling of words to their meaning and etymological derivation—surely a far more effective method of combating poor spelling than the enforcement of attention to the unrelated perceptual elements of words.

The Measurement of Visual Acuity. By R. J. LYTHGOE, with assistance by DOROTHY E. CORKILL and with a section by E. S. PEARSON. Medical Research Council Special Report Series, No. 173. London: H.M. Stationery Office. 1932. Pp. 85. 1s. 6d. net.

The general problem of this monograph is the relation between visual acuity and illumination, and its treatment is distinguished, not only by most able experimental technique and lucid exposition, but also by a recognition of the many psychological questions that are involved. Lythgoe shows clearly the importance of the end-point criterion that is adopted in acuity experiments, the effect of the brightness of surroundings on visual acuity, the influence of glare, the precautions necessary in the use of test-types, and the significance of observers' mistakes in this type of experiment. A statistical section is added by E. S. Pearson. Both for the importance of the results obtained and as a model of scientific presentation, this report deserves wide study among psychologists who are concerned with a sound biological development of their science.

Untersuchung der Entwicklung des Zeichnens. Von H. GRAEWE. Halle: Hermann Schroedel. 1932. Pp. 180.

Dr Graewe carried out experiments in infant schools, in schools for older children—up to the age of 14 years—and with a few adults. He studied, under various conditions, the reproductions of common objects, geometrical drawing and drawing from imagination. The result is a comparative study of capacities and tendencies in graphic representation which is of considerable interest. Graewe points out that an initial scrawling or scribbling stage which has hints of definite shape and representation within it develops gradually into a stage of genuine representation. He discusses in detail the relation of the different kinds of drawing which he studied to these stages. The book contains an excellent bibliography.

The Psychology of Literary Appreciation. By D. C. GRIFFITHS. Melbourne: Melbourne University Press (Oxford University Press). 1932. Pp. 142. 4s. net.

This monograph is published by the Australian Council for Educational Research. The topics dealt with are: the Origins of Literature; Reason and Imagination in Literature; Imaginative Activity during Appreciation; the Aesthetic Mode of Experience; Literature as Relaxation and Adjustment; Expression; Details of Form; the Sense of Beauty; the Influence of Environment; Educational Significance, and the Experimental Approach. Most of the discussion is on a general plane, but it is clear, suggestive and well expressed. A programme of research is formulated.

Du Cheminement de la Pensée. Par ÉMILE MEYERSON. Paris: Félix Alcan. 1931. L'ouvrage complet en 3 volumes. Pp. xxvii + 1036. 130 fr.

It is wholly impossible, within the limits of a short note to give any adequate account of this learned and brilliant treatise. M. Meyerson has written a continuous argument extending over more than 700 pages, and has added notes covering more than another 250 pages. His general problem is how the way of thought, which is fundamentally the way of identity, stability, order, can be reconciled with the way of sensation which is that of diversity, change and apparent disorder. His outlook and his problems are primarily logical and the psychologist will not find very much that deals directly with his particular realm. But of necessity many psychological problems are involved, and the student of psychology whose mind turns towards logic will find much to attract and stimulate him in this massive work.

Experimental Psychology. By HUBERT GRUENDER. Milwaukee (New York, Chicago): The Bruce Publishing Co. 1932. Pp. xiii + 455. \$2.50.

The contents of this book consist mainly of a selection from the experimental work which has been performed on the cognitive processes. Much of the material dealt with is important and interesting, more particularly the work of the Wurzburg school on the higher thought processes and upon the will. Unfortunately the book is also a polemic against sensationalism; and the biased attitude of the author, his dogmatic statements (this applies also to his treatment of instinct), and his one-sided selection of data do little to help the development of the critical attitude which he desires in his students. His use of italics is as infuriating as it is unnecessary.

Morfologi og Syntax: Nye Bidrag til Sprogets Theori. Af VIGGO BRØNDAL. Copenhagen: G. E. C. Gad. 1932. Pp. xvi + 108.

Mileu und Selbstbestimmung in der individuellen Entwicklung. Von WALKER POPP. Langensalza: Hermann Beyer und Söhne. 1930. Pp. viii + 166. RM. 4.30.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

- March 10, 1933. General Meeting followed by Lecture (see below).
April 6, 1933. General Meeting, arranged by the Aesthetics Section. "Why do we act and how do we act?" By Miss ELSIE FOGGERTY.

LECTURES.

- March 10, 1933. "Some Psychological Factors in Religious Experience." By Rev. Professor E. S. WATERHOUSE, D.D., M.A.

SECTIONAL MEETINGS.

MEDICAL.

- February 22, 1933. "Psycho-synthesis" or "Evolution in the Light of Freudian Psychology." By Mr C. C. FAGG.
March 22, 1933. "An Appreciation of the Debt which the Dentist of the Future will owe to the Psychiatrist." By F. W. BRODERICK, M.R.C.S., L.R.C.P., L.D.S.
April 26, 1933. "Emotion Styes and Tarsal Cysts." By W. S. INMAN, M.B.
"Psycho-Neurotic Syndromes in Childhood." By Dr MELITTA SCHMIDEBERG.

EDUCATION.

- March 6, 1933. "The Drawings of Mental Defectives." By Dr C. J. C. EARL.
May 1, 1933. "The Relation of Non-verbal Imagery to Learning." By Miss A. M. JENKIN.

INDUSTRIAL.

- March 9, 1933. "Market Research." By Dr G. H. MILES.
April 27, 1933. "Legibility in Print." By Miss M. D. VERNON.

AESTHETICS.

- February 24, 1933. Discussion on "The Objectivity of Aesthetic Judgment." Opened by Miss M. BODKIN.
March 17, 1933. "Where Does Psychology Touch Music?" By Dr P. BUCK.
April 6, 1933. General Meeting of the Society, arranged by the Aesthetics Section (see under General Meetings).
May 5, 1933. Discussion on "Art and Autobiography." Opened by Professor LASCELLES ABERCROMBIE.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

SCOTTISH BRANCH.

SESSION 1932-33.

- November 5th, 1932. Prof. H. READ. "The Psychological Significance of Recent Paintings by Picasso."
Prof. F. A. E. CREW. "The Problem of the Inheritance of Educability."
- December 3rd, 1932. Dr R. A. HOUSTOUN. "Demonstration of apparatus for viewing photographs under conditions which eliminate errors of size and perspective."
Dr S. DAWSON. "The Sigmagram."
Dr R. H. THOULESS. "Perspective in Oriental Art."
- February 4th, 1933. Dr C. FOX. "The Mind-Body Relation."
Dr B. P. WIESNER. "Maternal and Sexual Drives in the Rat."
- March 4th, 1933. Dr A. McNIVEN. "A Clinical Demonstration at the Glasgow Royal Mental Hospital."
Mr G. SETH. "Some Problems of Stuttering."

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.
2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.
3. The Society shall be responsible for the publication of *The British Journal of Psychology*.

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.
5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.
6. Honorary Members shall be entitled to all rights and privileges of membership.
7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.
8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.
9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.
10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology*.
- 10 a. Transfer from Sectional Membership to Sectional Association may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.
11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express dis-

approval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, one representative of the Committee for Research in Education to be nominated by that Committee, and of six Ordinary Members, and not more than two members co-opted by the Council.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election. Co-opted members of the Council shall cease to hold office at the Annual General Meeting of the year of their election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 a. *Nominations of representatives of the Sections, the Branches and the Committee for Research in Education shall be sent to the General Secretary at least two weeks before the Annual General Meeting.*

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council *and Committees* or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 21, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 2s.

36. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

37. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 a. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 b. The minimum number of Members necessary to form a Branch shall be 10.

46 c. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

COMMITTEE FOR RESEARCH IN EDUCATION.

This Committee has completed its annual enquiry as to psychological research upon problems having an educational bearing. The following is a summary of the replies received.

RESEARCH PLANNED OR IN PROGRESS, DECEMBER, 1932.

BINGLEY. THE TRAINING COLLEGE.

The Education of Physically Defective Children.

UNIVERSITY OF BIRMINGHAM. DEPARTMENT OF EDUCATION.

1. An Investigation into Methods of Training Teachers in some European Countries.
2. Tests of Higher Mental Processes.
3. Studies in the Psychology of Early Childhood.
4. An Enquiry into Tests of Specific Abilities needed for Secondary School Work.
5. An Experimental Inquiry into the suitability of the Otis Group Tests of Intelligence for use with Matriolated Students.
6. Preferences of Children and Adults in Poetry and Prose Literature.
7. Problems of Colonial Education.
8. Correlation between performance in English Composition and Intelligence Tests.
9. Errors in English Composition as revealed in Essays written by Higher School Certificate Candidates and University Post-Graduate Students.
10. Sex differences in Early Childhood in Spatial Observation and Visual Imagery.
11. Formal Training: an Historical and Critical Review with a re-examination in the light of Modern Psychology.
12. The motives of English State Education, considered in relation to the Problem of Educational Equality.
13. The Psychology of Public Appeal.

COLCHESTER. ROYAL EASTERN COUNTIES INSTITUTION.

Identification of Causes of Mental Deficiency.

CREWE. CHESHIRE COUNTY TRAINING COLLEGE.

1. Value of Training.
2. Difficulties in the Work of Young Teachers.

DURHAM. NEVILL'S CROSS COLLEGE.

Wishes of Adolescent Girls with regard to their Future.

EDINBURGH. ST GEORGE'S TRAINING COLLEGE.

Rhythmic Ability.

UNIVERSITY OF EDINBURGH. DEPARTMENT OF EDUCATION.

1. Binet Tests in two Indian Languages.
2. A Group Intelligence Test for Natives of Africa.
3. An Oral Group Intelligence Test for a wide age range, for English-speaking Children.
4. Continued work on the Moray House Group Tests of Intelligence, of attainment in Arithmetic, and of attainment in English, for eleven year olds.
5. Statistical Inquiries arising out of the Scottish Mental Survey of Children born in 1921, mainly questions of Selection and Sampling.
6. Statistical Methods of handling Examination Marks.

HULL. MUNICIPAL TRAINING COLLEGE.

1. Mental Testing of Students—Individual and Group. Comparison of Results with the Students' College Record and final Examination Results.
2. Investigation in "Racial Consciousness in Children."
3. Individual Child Study of Normal Children. Special attention to Factors of Home and Social Environment. All studies based on personal observation of children.
4. Selective Studies of Backward Children.
5. Comparative Studies of Groups of Children, graded for special ability, including English, Jewish and 'Black' Children.
6. Investigation in Children's Humour and Children's Ideals.

HULL. THE TRAINING COLLEGE, BEVERLEY ROAD.

1. Investigation of Visual Imagery and its place in Children's Learning (*continuation*).
2. Colour Preference in Children. aet. 9-10.
3. Testing of such Specific Abilities as lie within the range of Standardized Sets of Intelligence Tests.
4. Vocabulary of School Children aet. 5-6.
5. Changes during School Life of Children's attitude to Delineation, Colour and Form.
6. Place of Formal Teaching of Music in the development of Musical Appreciation in Elementary School Children aet. 12-14.

UNIVERSITY OF LEEDS. DEPARTMENT OF EDUCATION.

1. The Distribution of Intelligence, as measured by Non-Verbal Tests, in different Schools.
2. The Reliability of Entrance Examinations to Secondary Schools.
3. An Experimental Study of Retentivity.
4. The Intelligence of School Children of some of the Castes of Travancore.
5. A Comparative Study of Physically Defective Children of Various Types.
6. Psychological Tests in relation to achievement in Geography.
7. The Conceptual Development of Adolescents.

LEEDS. YORKSHIRE TRAINING COLLEGE OF HOUSECRAFT.

Investigation into the Origin and Nature of the special abilities involved in Craftsmanship with special reference to Housecrafts.

UNIVERSITY OF LONDON. BEDFORD COLLEGE FOR WOMEN.

1. Emotional Development in Young Children (*continuation*).
2. Eyedness and Handedness (*continuation*).
3. Part Played by Imagery in Learning (*continuation*).
4. Occupational Analysis: Nursing (in co-operation with the National Institute of Psychology) (*continuation*).

LONDON. EAST LONDON CHILD GUIDANCE CLINIC.

1. Temperamental Difficulties in relation to Behaviour Disorders (to appear shortly in the *Brit. J. Educ. Psychol.*).
2. Manifestations of Delinquency in relation to Position in Family.
3. Spontaneous Drawings of Nervous Children in the Play Room.

UNIVERSITY OF LONDON. GOLDSMITH'S COLLEGE.

1. Scientific interests of the Junior School Boy.
2. The Development of Humour in Nursery School Children.
3. Social Factors influencing the Educational Progress of Primary School Children.

UNIVERSITY OF LONDON. INSTITUTE OF EDUCATION.

1. The Testing of Children's Aptitudes for certain School Subjects.
2. The Fundamental Concepts of (a) Mathematics, (b) Science, (c) History; Tests to Estimate the Child's grasp of such Concepts; Variations with Age.
3. The Scientific Interests of Non-Science Pupils of School Leaving Age.
4. The Effect of Weekly Tests of Literary Appreciation on the Pupil's Ability in Composition.
5. The Effect of Speech Training on Oral and Written Expression in the Mother Tongue.
6. The Relative Influence of Home and School on the Literary Taste of Children.
7. The Relative Influence of Home and School on the Development of Character.
8. An Enquiry into the Geometrical Concepts of Young Children.
9. The Correlation of Abilities in Elementary Mathematics.
10. A Comparison of the Metrical and Non-Metrical approach to the Teaching of Geometry.
11. The History of Experimental Pedagogy in England during the past 50 years.
12. A Critical Analysis of Investigations into the Teaching of English in England and America.
13. A Statistical Analysis of Entrance Examination Marks (Scholastic and General Intelligence) and the Subsequent Progress of Pupils in certain Secondary Schools.
14. The Transfer of Training from the Inculcation of 'Scientific Method' in the Science Lesson.
15. Prognostic Tests of Teaching Ability for Students entering a Training College.
16. The Effect of Self-Criticism on the Pupil's Appreciation of Art.

UNIVERSITY OF LONDON. KING'S COLLEGE.

1. The Automatisation of Voluntary Movement.
2. The Perception of Tachistoscopically Exposed Symbols.
3. The Influence of Time Intervals on Recall.
4. The Influence of Volition on Retentivity.

5. Mental Factors in Aesthetic Creation and Appreciation.
6. Blood Distribution in Conative and Other States.
7. Visual Organization and Recall.
8. The Influence of Volition upon Thinking.
9. The Frustration of Will-acts and Conation.
10. The Psychology of Evidence. Comparison of Testimony in Normal and Delinquent Youths.
11. Perseverance and Perseveration.
12. Friendship-Love in Adolescence.
13. Volitional Attitudes.
14. The Influence of Affection and Will on Ergographic Performance.

LONDON. NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

1. Research in Connection with Vocational Guidance.
Experiments on Guidance for Boys in Borstal Institutions.
Experiments on Guidance in Schools at Willesden.
Experiments on Guidance in Schools in Fife.
Experiments, in collaboration with the Birmingham Education Committee, on Vocational Guidance, with particular reference to the Engineering Trades. Two reports have recently been published by the Birmingham Education Committee: *The Value of Vocational Tests as aids to the Choice of Employment* and *the Selection of Skilled Apprentices for the Engineering Trades*.
Research on Methods of Assessing Character and Temperament Traits.
2. Analyses of the Qualities and Abilities required in Nursing, in Teaching (Women), and in Secretarial Work (Women).
3. The Nature and Measurement of the Mental Abilities involved in Factory Assembly Work. An account of this research will be published in 1933, probably in book form, under the title of *Manual Assembly*.
4. The Influence of Rhythm on Occupational Motor Activities.
5. Tests for Motor Drivers.
6. Tests for the Ability to distinguish between Slightly Different Shades of Colour. A report on this work will soon be published, probably in book form, under the title of *The Measurement of Colour Discrimination*.
7. A Comparison of the Histories, etc., of Groups of Employed and Unemployed Lads. An account of this work has recently been published by Allen and Unwin under the title of *Occupational Misfits*.

LONDON. ST MARY'S COLLEGE, STRAWBERRY HILL.

The After School Careers of Boys who have been Prefects in Elementary Schools.

UNIVERSITY OF LONDON. UNIVERSITY COLLEGE.

1. Affective Influences in Intellectual Output.
2. Individual Tests of Oscillation.
3. A Study of Oscillation.
4. The Role of Specificity in Tests of Oscillation.
5. Fluctuations in Short Bursts of Work.

6. Comparative Value of Certain Verbal and Perceptual Tests and their relation to tests of Practical Ability.
7. Individual Differences in Reproduction of Completed and Uncompleted Test-items.
8. Transfer of Training and Linguistic Ability.
9. Linguistic Development in Young Children.
10. The Problem of a 'Spatial' Factor.
11. Relation between 'Form' and Specificity in 'Reference' *g*-tests.
12. The Significance of *O*-factor and *P*-factor as measurements of Introversion-Extraversion (part of a larger study on the Nature of *O*-factor and *P*-factor).

MANCHESTER. THE UNIVERSITY.

1. Experimental Investigation of Criteria of Effective Speaking.
2. Art-forms of Speech.
3. An Attempt to bring about Artificial Synaesthesia.
4. The Factors involved in Auditory Localization.
5. The Nature of 'Shyness' and its relation to other Mental Traits, *e.g.* Left-handedness and Speech Defects.
6. An Experimental Inquiry into Colour Discrimination.
7. The Relationship between Intelligence Tests, Performance Tests, Manual Dexterity Tests and Mechanical Ability Tests.

UNIVERSITY OF MANCHESTER. DEPARTMENT OF EDUCATION.

1. An Investigation into the Relationship between Backwardness in Speech and Mental Retardation.
2. Experiments on the Usefulness of Partial Hearing, especially in Relation to the Development of Speech and Language in partially Deaf Children.
3. Methods of Measuring Auditory Acuity, the Occurrence of Auditory Fatigue, and Problems in Connection with Hearing Aids.

NEWCASTLE-UPON-TYNE. ARMSTRONG COLLEGE.

1. An Enquiry into the Memorization Values of Various Methods of Teaching Geography.
2. The Effect of Subconscious Association in Intelligence Testing.
3. The Diagnostic Value of Tests of Perseveration among School Children.

NOTTINGHAM. UNIVERSITY COLLEGE.

1. Development of Language in Young Children.
2. A Survey of Adult Education of the County.

READING. THE UNIVERSITY.

Relation between Arithmetical and Geometrical Ability.

UNIVERSITY OF READING. EDUCATION DEPARTMENT.

The Teaching of Divinity in Senior, Central and Secondary Schools.

UNIVERSITY OF SHEFFIELD. DEPARTMENT OF EDUCATION.

1. Analysis of the Learning Curve.
2. The Development of Moral Values among Senior School Children.
3. Mental Ability and Correlation Coefficients.
4. Children's Vocabularies.
5. The Effects of Alternative Modes of Presenting Geographical Information.
6. Mechanics Institutes and their contribution to Scientific Education in England.

UNIVERSITY COLLEGE OF SOUTH WALES. CARDIFF.

1. Continuation of Investigation into the use of Intelligence Tests in Secondary School Admission Examinations.
2. The use of Performance Tests in the Investigation of Problems of Bilingualism.
3. The Psychology of Artistic Creation with particular reference to Literature.
4. Literary Appreciation.

ON CONCEPTUAL THINKING.

By A. W. WOLTERS.

(From the Psychological Department, University of Reading.)

- I. *Concepts are not mental entities* (pp. 133-4).
- II. *How do we think?* (pp. 134-5).
- III. *The problem of universals* (pp. 135-6).
- IV. *Universals as schemata* (pp. 136-8).
- V. *Abstract thinking* (pp. 138-9).
- VI. *Schemata as objects of reaction* (pp. 139-42).
- VII. *Problems for discussion* (pp. 142-3).

I. CONCEPTS ARE NOT MENTAL ENTITIES.

THE substance of this paper was presented for discussion on several occasions¹, and I have endeavoured to profit by the criticism to which it was subjected. My chief debt, however, is to Prof. Bartlett, whose recently published book, *Remembering*, has done much to strengthen my convictions and to clarify my views. It would be improper to claim his support for the conclusions stated below, but the success with which he has applied a similar method in an allied sphere encourages the hope that the psychology of thinking may be advanced along these lines.

The term *concept* and its derivatives are here used in the traditional sense, without precise definition. We assume that the many thinkers who have discussed the concept have had in view the same topic, however differently they have defined it. To select or formulate a precise definition at this stage would be likely to beg the question, and would also run counter to the hypothesis which is to be put forward. But the heart of the problem which has vexed philosophers and psychologists alike is the fact that we are able to think in a manner termed universal or general. It is not necessary here to do more than refer to the classical discussions of this problem, and to the long series of experimental researches which have been directed to it; and to venture the statement that the psychological treatment has been hampered by the logical preconceptions from which it started, and by the tacit assumption that it must run parallel to logical

¹ At meetings of the British Psychological Society and of the Cambridge Psychological Society, in March 1932 and February 1933 respectively.

theory. We have been left with one clear truth and several prejudices. The truth is, that in some sense of the term we do think universally. The prejudices are, that concepts exist as mental entities, that they can be present to consciousness, and that all that is required is to describe them and to trace their genesis. The laborious efforts to lay bare the phenomenology of the thought-process have brought to light material of great interest, but it may be questioned whether they have done much to elucidate the nature of thinking as an active process. In particular it must be questioned whether the imageless, or pure, thought-process helps greatly to a solution of the difficulty. Some writers have maintained that the introspectible presence of images yields no evidence as to their function. The same is surely true of more modern substitutes for images. In any case, they are inevitably dated mental events, and as such particular, and they cannot of themselves suffice to explain the generality of the thinking in which they function. Further, the only justification for calling them concepts seems to have been that nothing else could be found to wear that label. Or perhaps the argument that the particularity of any representational image disqualified it from being a general concept, led to the acceptance of the corollary that a process which was like nothing at all must be general.

II. HOW DO WE THINK?

The problem may be more manageable if we are content to put aside phenomenology for the moment and consider *how* we think rather than *what* we think. To start from the obvious, thinking is a reaction to a set of circumstances which demand a response by the organism, though the response need not issue in muscular activity. We thus bring it under the more general heading of behaviour (since we need not submit to the appropriation of this useful and common word by any particular school). Then it seems reasonable to consider how far thinking presents features common to other forms of behaviour, before we draw the conventional distinction between practical and theoretical activities. There are important differences between sawing wood and calculating, but so there are between sawing and rowing. We do not regard the latter pair of activities as so unlike that we cannot treat them under a common head, and there seems no reason why we should at the outset separate the members of the other pair. Every effective activity is determined by the nature of the problem exciting it, and by the materials available for the solution. It does not follow that each must be considered psychologically unique. The point that needs to be stressed here is that thinking is conative, a prin-

ciple to which we pay a ready lip-service while neglecting its implications. Let us then say that thinking is a mode of behaving, and allow the specific differences to come into view later.

III. THE PROBLEMS OF UNIVERSALS.

If this is accepted we find the problem of universals confronting us in animal behaviour as certainly as in human. A dog recognizes any dog as a dog and any cat as a cat, as is clear from the appropriate direction of activities towards members of the two species. It is difficult to suppose that the animal himself has before his mind at any stage such a psychological entity as a concept of 'dog.' And in this case we humans seem to be no better off, for it would puzzle any one of us to say what we mean by the word *dog*, except by pointing to animals which we know to be so called. The *Concise Oxford Dictionary* is content to define a 'dog' as a "quadruped of many breeds wild and domesticated." On introspection I can find nothing in consciousness except the word, and a sense that I know something of dogs and can deal with them, but it is impossible to state what it is that I know. Yet both the dog and myself are dealing with universals. The dilemma now seems to be that either the concept of *dog* must be denied to ourselves, or we must allow it to the animal. One statement must cover the two cases. But if we accept the latter alternative, the concept cannot be a psychological entity before consciousness.

The universal is found in the behaviour of the creature. This rests upon a tendency which is awakened in certain circumstances. But *any* cat will excite it; all cats are equivalent in this respect. There is a readiness, or preparation, for activity, and it is general in the sense that it is available for a number of situations not completely identical. The animal universalizes in consequence of the flexibility of its prepared behaviour, and the preparation is a character of the organism. Where we find this incompletely specialized readiness it is perhaps permissible to call the resulting behaviour 'conceptual,' in the case of animals as much as of human beings. The name is less important than the recognition that there is a mode of reaction common to men and animals which must be accepted as general in character, but which does not involve the recall to consciousness of any content. It is something just short of habit. Indeed, the term *habit* is often applied to reactions of this kind, but, I think, improperly. This is a word which is employed with inconvenient looseness. It has been applied to such activities as that of *Convoluta*, which emerges from and retreats to its burrow in almost perfect agreement with the tidal rhythm for some days after it has been withdrawn from the

tide's influence; and also to what are called 'business habits.' In the former case there is exact repetition of the activity both in general configuration and in detail, while in the latter the pattern is imperfectly repeated and the details may be new each day. Yet there is something constant in the business man's routine. The most profitable line of approach appears to be offered by Prof. Bartlett's conception of schemata.

IV. UNIVERSALS AS SCHEMATA.

In habit proper the organism is prepared to deal with one set of circumstances whenever they recur, its parallel being the rigidly determined instinctive reactions of the simpler organisms. But the dog displays a readiness (*a*) to deal with different but equivalent situations, as when the cat or fox is changed, and (*b*) to vary the details of the activity to meet variations in circumstances, for no two hunts will be the same. In respect to fixity of response there is something less than habit, but in respect to efficiency something more. The theoretical importance of these familiar facts is that they point to the need of inserting between trial-and-error behaviour and habit a mode of preparation for reaction different from both. One is tempted to explain the facts by saying that a frustrated habitual activity is helped on by trial and error, but though this undoubtedly occurs at times, and may possibly account in some measure for the development of the generalized tendencies under discussion, it cannot be accepted as a complete explanation in view of the prompt and immediate character of the variant reaction in many instances. A specialized tendency would lead to fumbling whenever the situation changed, but these can be very sure as well as flexible. Their distinction from the tendencies which issue in habitual actions is that they are schematic (though not indefinite). At the level of behaviour on which the discussion is now moving, it appears to be schemata of reaction tendencies which provide the universal moment, by permitting differing objects to be treated as equivalent modes of satisfying the organism's needs. The schemata can only be deduced from the actual behaviour, but probably we should conceive them as existing continuously, as characters of the organism, but not as entities which the organism 'possesses.'

Let us now consider more highly developed human activities. I arrive at night at a station in an unfamiliar town, and finding that it is raining, take a taxi to the address which has been given me. All the physical setting of my actions is new, so that no fixed routine is available. Yet I am prepared to cope with the situation, and carry out the necessary actions with a minimum of hesitation and inconvenience, while thinking

more of the business which has brought me there than of the new environment. I enter one particular cab, but it was not a particular one which I sought. Any one would serve. All were equivalent in relation to my need. If a suitable bus arrives, I enter that instead, and thereby treat that as equivalent to a taxi. All that concerns me is the function of the objects, and this function in turn depends more on my purpose than upon the vehicles. If it had been a sunny day none of them would have had a function in my universe. But behind the purpose stood a schematic preparation for action developed from past experiences. It is the combination of my purpose to reach my destination dry with the schema, which leads to the identification of vehicles of widely different forms, and through that combination I universalize.

This behaviour is quite unreflective. Except the conscious intention to avoid the rain and reach my destination, there is nothing 'before the mind.' It is only the dependence upon an elaborate previous experience which distinguishes it psychologically from the dog's hunting. Nevertheless, it apparently implies what would be termed by the logician concepts. But if concepts can be recognized at all in such activities they can only be found in the pattern of the behaviour, and the most distinctive feature of that was the readiness to set about it, it being in other respects like an entirely new and unique action (if there can be such). Consequently, if we are to admit the term 'concept' at this stage it must be applied to that flexible organization of reaction tendencies which provides a schematic preparation for various activities directed to the satisfaction of a recurring need. This may be expressed more briefly in popular terms by saying that the concept consists in 'knowing what to do about it.' More precisely, at this level a concept is a *skill*. It need not be represented in consciousness, as when in playing chess we move the pieces in the legitimate manner without thinking of the rules.

We may now test this conception on reactions of a higher kind. When I recognize an object as a tree a rudimentary conation is involved, since the recognition implies a latent readiness to react appropriately to it, mentally or physically. The primary reason why both ash and elm are regarded as trees is that the same reactions will serve for each, and they only become differentiated when specific needs arise. At the moment no overt reaction may be needed, but the object is safely placed and subjected to the schema. I find a flower which is new to me and examine it until I realize that it is a crane's bill. It appears from introspection that no recalled material, imaginal or otherwise, is necessary in such a process. The process was conational and directed, and depended on a concept of

the genus and also on that of natural orders in general, but these concepts need not be represented in consciousness until the actual moment of decision, though the action was clearly prepared in part. I may be described as knowing how to identify the plant, but, during the process, that knowing is of the same order as when one knows how to play the violin. The organized tendencies determine the reaction without obtruding themselves as moments in it. The concept directs activity without being an object of thought, and its value lies in that function. But we must turn now to examples which come nearer to abstract thinking.

V. ABSTRACT THINKING.

No process deserves the name of thinking unless it is directed to a problem, to a situation demanding reaction when no reaction is fully prepared. We seldom think earnestly because most situations can be met by preformed tendencies; we constantly think a little because most situations contain a novel element. The hypothesis here advanced is that the notion of schematic preparation can be usefully applied in explanation of general or universal thinking. The essential psychological character of a concept consists in being ready to deal with this or that. We may take for example one of the most abstract and general modes of thinking, that which involves numbers. The reader is invited to attempt to answer in turn the following questions, and to observe the process of solution:

How many chairs are there in the room?

What is the sum of five and twelve?

Can there be an infinite number?

What is number?

We can all answer the first and second questions, and children would be no less efficient. We can all provide an answer to the third, though our replies may differ. But the fourth will leave most of us dumb. Nothing seems to rise to consciousness that can be gripped. Yet it must be illogical to answer the other questions while ignorant of the answer to this. Must we admit that we have no concept of number? That is absurd, since we displayed such efficiency in regard to the other tasks. And how could we contemplate the present difficulty unless we could think of number as such? The solution of these problems is that psychologically the concept of number consists in being able to think numerically, as when we count or add. If we can do so much competently, we possess an adequate concept of number, however little we may be able to say about

it. In its most immediate and most important sense, the number *two* is a dodge for treating this object and that conveniently. It is a mode of thinking (and thinking is behaving) but not something thought about. It is, however, genuinely universal. It is not in its primary character an object of thought, but an instrument of thinking. To borrow Bartlett's happy phrase, it is not that which we react to, but that by means of which we react. It is a schematic readiness to think numerically. It fails when we come to the fourth question because that does not present a situation of the type to which this schema is directed, but one in which skill in numbering is of no avail. The problem set relates to our behaviour when numbering and not to things to be numbered, and is therefore psychological rather than mathematical. Thus we have come to envisage a hierarchy of activities, for we are now thinking of how we think when thinking numerically. An activity and its organized determinants have become the object of another activity which demands the organization of a new system of reactive tendencies. That by means of which we react has become that to which we react, and the two levels of reaction are functionally distinct. Absence of a schema at the higher level, or inability to form one, is no evidence of the inadequacy of the lower level one, for the concept active in thinking is never the concept thought of. Whatever their logical relations may be they must be considered as psychologically distinct.

It may be noted in passing that many problems are of a double kind, since the need for communication is added to the need of solving the question posed, and brings into play another system of schemata. The intimacy of the connection between language and thinking tends to obscure the distinction between the two reactions, but really two levels must be accepted. The reaction of solution becomes part of the situation to be dealt with by the speech reaction. This, it must be admitted, is a very theoretical distinction, but it is worth while to draw it, and to conclude therefrom that inability to make the solution clear to an audience does not prove that the speaker had not attained a solution.

VI. SCHEMATA AS OBJECTS OF REACTION.

We have been led to distinguish sharply the performance of an activity from the subsequent questions as to what we have been doing and how we did it. In another field Prof. Bartlett has said that the organism 'turns round upon its schemata.' This is not a perfect description, though it is difficult to find a better one. Undoubtedly something of this kind occurs. We must be on our guard against taking the schemata as

entities and setting them over against the organism which has them, as things distinct from it. As already stated, they are modifications of the organism. Now we can emend the description and say that the organism turns round upon, or reacts to, *itself as schematized*. Then we can construct a series of instances of organisms reacting to themselves, from the kitten pursuing its tail, through the valetudinarian revising his diet, to Socrates examining his concept of justice. In an important sense this series is continuous, and from some points of view it is advantageous to regard it as such, for we thereby avoid the mistake of treating thinking as something unique and very mysterious. Nevertheless, reflection upon the manner of one's behaving appears to be an exclusively human activity, and thence arises the problem as to how the schemata are made available and manageable as objects of reaction. Here phenomenological studies come into their own, and the wealth of data already accumulated take on a new significance. It may be that the imageless thought is of peculiar importance in this process, though I confess that I am still unsatisfied that imagery has no function of importance, or that *Wissen* always have a significant content. To my introspection they often appear reducible to an awareness, or confidence, that I can deal with the situation, a confidence which frequently proves to have been ill-founded. The nature of the psychological material is, however, a subordinate question (though one of great importance and interest). The psychological laws of thinking are not to be deduced from the material which it employs. Further, to revert to the original problem, universality cannot be a content, but only a function of a content. No process can be psychologically universal in itself; perhaps *any* process may be universal in its function. That is only to say that it can be employed in the solution of a number of problems, as the schema controlling it can meet varying situations. Any effective schema must be sufficiently fluid to permit variation, since mere repetition is seldom adequate (even in such a routine activity as shaving), and from this we may conclude that the distinction between the universal and the individual has no great psychological interest. Restricting the term conceptual thinking to reactions *by means of* concepts, no matter what the object to which the reaction is directed, we may say that the difference between habitual and conceptual activities is one of degree of flexibility.

In the more abstract reaches of thought, where we think about concepts (that is, react to schemata) the problem is renewed without changing its character. The situation is different, the materials in which it presents itself, and those by means of which it is brought under control,

are different, but the psychological outline remains the same. The process often proceeds by blundering trial-and-error efforts which are largely experimental in character. Our memory of efforts to solve geometrical riders will furnish ample illustrations of this. We carry on, as it were, a guerilla warfare until we have mobilized and organized adequate resources. In nearly all cases some schemata are available, but not the one necessary for the solution, and those of service in the reaction are not those which constitute the situation to which we have to react. Nothing but confusion can result from ignoring the psychological distinction between the object of thought and the instruments by which we think. We must repeat that the object to which thought is directed is not a mental entity: it is a form of behaviour. The conscious processes by which that form is represented are, in their turn, instruments by which the organism seeks to make its schemata available for further organization. This complication of conative processes must be faced: we have no right to expect that abstract thinking can be explained simply. In its more creative moments it utilizes what means it has to forge a new instrument.

Abstract discussions of goodness and beauty are not directed to the good and the beautiful, but to our behaviour in regard to objects to which we apply those epithets, and so occur in a sphere from which the qualities have been banished. It would be unbecoming to suggest that in like manner the truth is not to be found in philosophical theories of truth, but it must be maintained that the truth of an argument is not psychologically the truth that is its subject-matter. Cheap Socratic victories are easily won by ignoring this distinction of levels. Is there any evidence that Socrates' victims were less competent to act justly than himself, though less practised than he in talking about their behaviour? He who can recognize truth and beauty when he meets their embodiments must be allowed to have formed the relevant concepts, the adequacy of which is indicated by the efficiency of his behaviour in circumstances such that they can operate, and not by his ability to review them in detachment.

A particular instance of abstract thinking deserves notice. The paradoxical consequences of setting thought to examine itself have often been remarked. The character of thought, it is said, is taken for granted even when one seeks, by processes of thought, to elucidate it. But there appears to be little difficulty for the psychologist in this. His concern is with the organism thinking, and we are now familiar with the case of an organism reacting to itself. The process of developing a theory of thinking (and let us suppose for simplicity that the thinker is depending entirely upon his own processes for data) is reaction to the schemata by which he

thinks when engaged with other problems. One test of any theory of thinking is that it should be able to account for its own existence, and the hypothesis set forth in this paper can claim to meet it by using itself as an example. The process is still one of 'turning round.' The difficulties encountered in making clear the nature of schemata (so far as they are not due to paucity of knowledge) are exactly what we should expect. A technique has yet to be developed for bringing the functions under conscious control of the organism, and for making them amenable to verbal description. When the reactions to this problem have been organized, a new schema will have been formed, which will be universal in the only sense that matters to us, for it will enable us to confront successfully those subordinate problems which constitute varying situations.

VII. PROBLEMS FOR DISCUSSION.

Metaphysical or epistemological implications and difficulties are not our concern. It is sufficient for us to agree that the universe must be such as to admit of universalizing, though the high-handed manner in which we set about it suggests that a study of objective characters is not likely to be of great profit to psychology. However much the material environment may assist it, the organism universalizes by its own activities. But although we are at liberty to avoid these difficulties it is not pretended that the development of thought-psychology along these lines will prove an easy undertaking. The detailed problems which have still to be met are concerned first of all with the nature of the schemata, the manner of their formation, and the laws of their activity. It may be found that other sciences, and especially logic, mathematics and grammar, have already done much of the work. There must also be considered the ready, unreflective, variation of behaviour to meet small changes in problem-situations. How can a schema be as fluid as it appears to be? Language appears to offer the readiest field for research on this matter. What differentiates the instances in which the organization is formed or is operative without any manifest conscious representation, from those in which insight is essential? The term 'insight' requires much more elucidation before it can be safely used by psychologists. And what is the nature of that curious moment of transition from uncertainty to conviction, that mental click which marks the successful termination of intellectual effort?

Against these difficulties can be set advantages other than the avoidance of controversies of doubtful relevance to psychology. The view I have advocated brings us back to the person thinking as the proper

topic of inquiry. By taking as the starting-point the fact that the thinker is engaged in mastering an obstruction, we are able to bring this activity into line with other instances of conation. For example, we may quote McDougall's classic description of instinct: "We may, then, define an instinct as an . . . innate psychophysical disposition which determines its possessor to perceive, and to pay attention to, objects of a certain class, . . . and to act in regard to (them) in a particular manner." This is as good a description of schemata as can at present be obtained, and, apart from the word *innate*, it will serve for concepts. The distinction to be drawn is between innate and acquired skills, the members of each class being further differentiated by the degree to which each is determinate or capable of varied application. In all cases the core of the organization of reaction tendencies is teleological. So, having restored thought to its full psychological context, we are enabled to utilize a wider range of data. We need no longer depend exclusively upon the introspection of highly trained (and often artificially trained) subjects, since the less precisely formulated descriptions of activities that all men offer from time to time can be pressed into service, while along much of our path we can observe the spontaneous behaviour both of men and of animals. Nor need we abandon any part of what the science has previously won, if we hold that conceptual thinking essentially consists in bringing to bear upon a presented situation a preformed schema of reaction tendencies, and that, if the substantive term *concept* must be retained, it should be used to denote the schema, and not any one form of introspectible content.

(*Manuscript received 22 May, 1933.*)

AN ASPECT OF HUMAN REASONING.

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(*From the University of Michigan.*)

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I. INTRODUCTION.

WHEN behaviour is blocked because a desired end is not at once attainable a problem arises. The nature of the resulting difficulty depends upon the way in which the problem is considered, and hence the mode of overcoming the difficulty varies from case to case. The particular attempt at solution which is adopted may be called the direction⁽⁴⁾ of solution, and most problems may be attacked in several directions. It is necessary to have a correct or workable direction if a problem is to be solved. When there are several difficulties, each of which can be successfully overcome, the problem will have several solutions⁽⁵⁾. The solution of a problem in each case is a pattern consisting of parts of past experience which have become integrated. These parts of past experience need never have been previously associated. A solution-pattern may be a spontaneous combination of isolated experiences⁽³⁾. In such a case the problem is solved by reasoning or productive thinking. When the solution-pattern is made up of past experiences which have previously been associated, then the problem is solved by reproductive thinking.

I have previously demonstrated that the formation of a solution-pattern is greatly facilitated if the reasoner is given a correct or workable direction by the experimenter(4). Some problems are rarely solved by laboratory subjects because the correct direction for such problems is not found by them. A particular false or unsuccessful direction may be common to many subjects and may determine all the activity. Analysis indicates that such common directions may reasonably be called 'habitual directions.' Thus if a hat-rack is to be built out of two poles and a clamp in the middle of a room; the 'habitual direction' prompts an attempt to build something with legs. In this case the 'direction' is unsuccessful and the habitual approach so much wasted energy.

Good reasoners do not indefinitely pursue unsuccessful approaches to a problem. They jump from one direction to another in their attempts to overcome first one difficulty and then another. Poor reasoners, on the other hand, continue in one direction and may attempt for hours to overcome the impossible. That the ability to reason is a general rather than a specific function has recently been demonstrated by Billings(5). He found that the ability to solve new problems in the various fields of knowledge was highly correlated, and that the ability satisfied the 'tetrad equation.' The ability to solve problems in a specific field was found to have little to do with familiarity with that field, provided the data necessary for the solutions of the problems were known to the reasoners.

Gottschaldt(2) found a difference in behaviour in a problematic situation between normal and feeble-minded children similar to that between good and poor reasoners. The behaviour of normal children can be characterized as variable, that of the feeble-minded children as lacking flexibility. Thus in a task which consisted of piling up blocks as high as possible the normal children varied their attempted structures and so were likely to hit upon a successful method. The feeble-minded children, on the other hand, continually repeated (although with increasing skill) their first attempts and so continued to fail.

In a study of reasoning in rats(6) I found a similar difference in the behaviour of normal and partially decorticated animals. The behaviour of normal rats is variable, adaptive, and free from control by previously formed habits. Rats with cortical injuries, though still able to learn mazes efficiently, completely lose their ability to reorganize their past experiences. Their behaviour becomes more and more stereotyped as the extent of the cortical loss increases.

II. THE PURPOSE OF THE PRESENT EXPERIMENT.

The purpose of the present experiment is to determine whether the habitual direction is a function which actually blocks the correct direction, or whether it is merely a function which is present during the absence of more fruitful processes. If the first alternative is true, it follows that general suggestions which warn against the danger of continuing on an unsuccessful approach should facilitate problem-solving ability. If the second alternative is true, such suggestions should be less effective. Only specific suggestions which influence the reasoner to take the correct direction would then be of much help. This would mean that general instructions are relatively useless, because each problem has its own particular successful direction. The present experiment is thus an attempt to see whether the removal of habitual direction by general instructions will cause adult human beings to be more successful problem-solvers.

III. EXPERIMENT I.

(1) *Method.*

The 384 students in an introductory course in psychology were divided into two groups (1) an experimental group and (2) a control group. The same problems were presented to both groups. The experimental group, however, received a 20 min. lecture followed by general hints on how to reason (the hints were actually a summary of the lecture) before being presented with the problems. Otherwise the conditions for both groups were the same.

The tests were made during the regular 3-hour laboratory period. There were seven such periods. The experimental group was made up of the first three laboratory periods, the control group of the last four laboratory periods. Thus if students tested during the first periods of the week communicated the solutions to students who had not yet been tested the control group tended to be favoured.

Individual work was required by all students. They were asked to guard their own work, to avoid seeing the work of others, and to refrain from saying anything to anyone about the work during the week. The students did not know the purpose of the experiment, but regarded it as one of the regular laboratory experiments. They could easily see that their introspective reports would be useless if the problems were familiar, and consequently co-operated in every respect.

(2) *The lecture and hints.*

Previous to the presentation of the problem the experimental group was given a lecture which covered the following points:

- (1) The solution of a problem, when it is the product of reasoning, consists of a pattern which is made up of parts of different past experiences.
- (2) The pattern forms suddenly as does the hidden face in a puzzle picture.
- (3) Meanings of elements depend on the pattern of which they are a part. The sudden formation of a pattern therefore results in sudden changes of meaning.
- (4) The solution-pattern overcomes a difficulty.
- (5) The difficulty is what one sees it to be. It is not in the problem. (Illustrations were given which show how the same problem can be solved in different ways, each solution being the conquering of a different difficulty.)
- (6) The particular difficulty one sees determines what one will do about it, i.e. what direction one will take (e.g. one doctor will seek a serum to immunize man to certain germs, another will seek a means for preventing the germ from travelling).
- (7) All difficulties cannot be overcome. Hence one must find a difficulty which can be overcome.
- (8) Most people see the same difficulty.
- (9) The difficulties we see are often determined by our past contact with problems (e.g. other diseases have been conquered by the discovery of serums). Such difficulties are habitual difficulties and give rise to habitual directions.
- (10) Habitual directions do not solve difficult problems. Problems are difficult when a successful direction is not obvious.

The hints on how to reason were as follows:

- (1) Locate a difficulty and try to overcome it. If you fail, get it completely out of your mind and seek an entirely different difficulty.
- (2) Do not be a creature of habit and stay in a rut. Keep your mind open for new meanings.
- (3) The solution-pattern appears suddenly. You cannot force it. Keep your mind open for new combinations and do not waste time on unsuccessful attempts.

(3) *Test problems and procedure.*

Three problems varying in their degree of difficulty were presented to both groups. The material for solving the problems was piled on a table which the students were allowed to inspect. All solutions, however, were described on paper. When a solution to each problem had been found the student presented it to an instructor. The instructor either (1) accepted the solution as correct, (2) rejected it as impractical, or (3) asked for a different solution. Each student continued his or her efforts until a solution to each problem was accepted or until the end of the allotted time.

The three problems were presented at the beginning of the test period. The students were permitted to work at them in any order they desired. One hour of effort was permitted.

Each of the problems was easily stated. The students were told that each problem had a good solution and that there were no 'catch' problems. They were told that the problems were difficult and required considerable thought, but that the information necessary for solving the problems was common knowledge.

The problems used were:

(1) *The string problem.* One string was fastened to the ceiling and was of such length that it reached the top of a heavy stationary table. Another string was fastened to the wall about 6 ft. above the floor and was of such length that it reached to the floor. The problem was to tie the ends of the two strings together. This was difficult because when either string was held the other was out of reach.

Simple solutions were demonstrated by the experimenter. These solutions are (1) hooking one string in with a pole; (2) increasing the length of one of the strings; and (3) tying one of the strings to a chair and placing the chair half-way between the cords in order to keep one string within reach while getting the other. These demonstration solutions were not to be used even in a modified form by the subjects.

The solution desired of the students was that of converting one of the strings into a pendulum.

(2) *The hat-rack problem.* Making use only of the material on the table, subjects were required to construct a hat-rack sturdy enough to support a heavy coat. It was to be built in an empty room of ordinary size.

The only available materials which were useful were two poles (between 6 and 7 ft. in length) and a table clamp.

The problem could be solved by clamping the two poles together and wedging them between the floor and the ceiling. The clamp could be used as a hook. (Cotton waste was present in case a student wished to protect the ceiling.)

(3) *The candle problem.* Lighted candles were set on a table. These were to be put out from a fixed distance of 8 ft.

Glass and rubber tubing ranging from 6 to 12 in. in length were present. These could be fastened together to form a long tube. By fastening this tube to one of the above-mentioned poles with sturdy paper clamps the flexibility of the tube could be overcome. The candle could then be blown out.

All problems were to be solved with the use of the materials on the table. In addition to the already mentioned material the table contained such things as washers, bolts, pliers and chalk.

(4) *Results.*

The results of 178 students in the experimental group and 206 students in the control group are given in Table I. In the second and third problems the experimental group shows a decided superiority over the

control group. This does not mean that the superiority is dependent on certain problems, but merely shows that the first problem received the greatest amount of attention. Fewer of the control group had time enough to turn to the other problems. The sum of the percentages of each of the three problems which were solved is therefore a more satisfactory score with which to make comparisons. The control group made a total score of 119.1 per cent., or solved 39.7 per cent. of the problems. The experimental group made a total score of 147.6 per cent., or solved 49.2 per cent. of the problems. If we consider the groups to be equal at the start the above results indicate that the suggestions helped the experimental group to make a score 24.1 per cent. higher than the control group.

Table I. *Scores made by experimental and control groups.*

Group	Number of students	Percentage which solved			Total
		Problem 1	Problem 2	Problem 3	
Experimental	178	50.6	28.7	68.3	147.6
Control	206	49.0	22.3	47.8	119.1

Table II. *Scores made by men.*

Group	Number of students	Percentage which solved			Total
		Problem 1	Problem 2	Problem 3	
Experimental	89	68.5	41.6	77.6	187.7
Control	114	61.4	35.9	57.0	154.3

Table III. *Scores made by women.*

Group	Number of students	Percentage which solved			Total
		Problem 1	Problem 2	Problem 3	
Experimental	89	32.6	15.7	59.0	107.3
Control	92	33.7	5.4	36.4	75.5

In Tables II and III the same data are treated separately for men and women. For both sexes the superiority of the experimental group is obvious. The control group of men made a total score of 154.3 per cent., the experimental group of men a score of 187.7 per cent. This is an increase of 21.5 per cent. The control group of women made a total score of 75.5 per cent., the experimental group a score of 107.3 per cent. This is an increase of 42.1 per cent. Thus the women seemed to profit much more from the suggestions than the men. These figures also show that the men in both groups were decidedly superior to the women in the corresponding groups. This sex difference may be real, it may mean that the superior

women avoid college, or it may mean that the problems were better suited to the men.

Table IV. *Scores made by superior and inferior reasoners.*

Group	Number of students	Percentage which solved	
		Problems 1 and 2	No problem
Experimental	178	21.9	16.4
Control	206	14.6	22.8

In Table IV results are presented to show the relative effect of the suggestions on poor and good problem-solvers. The first two problems are obviously the more difficult ones. Individuals who solved both of these problems may be regarded therefore as superior reasoners. Those who solved none of the problems are inferior reasoners. The table shows that the control group contains 14.6 per cent. superior reasoners, whereas the experimental group contains 21.9 per cent. superior reasoners. The experimental group thus contains 50 per cent. more such individuals than the control group. On the other hand, the control group is made up of 22.8 per cent. inferior reasoners and the experimental group 16.4 per cent. Thus the control group contains 39 per cent. more inferior reasoners than the experimental group.

These figures strongly indicate that the general suggestions were decidedly beneficial to both poor and good reasoners. They do not explain why the women profited more from the suggestions than the men. Thus some factor other than the poorer score made by the women must account for the fact that they profited more from the suggestions than did the men.

IV. EXPERIMENT II.

(1) *Procedure.*

The results of Exp. I show a marked superiority of the experimental group over the control group. This superiority may be due to an original inequality of the groups. It therefore seemed desirable to vary the experiment in such a way as to measure one group of individuals in its ability to solve a control and an experimental problem.

Accordingly three problems were devised which could be easily solved by individual work. Only two of these problems were necessary for the experiment, the third was used in case one of the other problems was familiar to certain students.

The time to be spent on the problems was divided into four 10-min. periods. Period 3 was a continuation of period 1, and period 4 of

period 2. The problem attempted during periods 1 and 3 was the control problem, the problem attempted during periods 2 and 4 was the experimental problem. Between periods 3 and 4 the lecture and suggestions were given. Between periods 2 and 3, 12 min. were set aside for rest and for the presentation of additional problems to be used by such students as had solved their problem during the first two periods. This interval preceding period 3 thus served as a control of the lecture interval which preceded period 4.

The number of solutions occurring during periods 1 and 2 shows the relative difficulty of the control and experimental problems. During periods 3 and 4 there is an increase in the number of solutions. The difference between the percentage increase in solutions during these two periods is a measure of the effect of the lecture and hints on the problem-solving ability.

(2) *The problems used.*

(1) *The square problem.* Given three-quarters of a square, divide the area into four parts which are equal in shape and size.

(2) *The dot problem.* Given three rows of three dots each, pass through each of the dots with four straight lines without raising the pencil from the paper. Retracing a line is regarded as making a line.

(3) *The 'T' problem.* Each student was given four blocks of wood. From these he or she was asked to construct a perfect 'T.'

One of the first two problems was to be selected for periods 1 and 3 and therefore became the control problem. One of the last two problems was to be selected for periods 2 and 4 and became the experimental problem. A few of the students had heard of two of the above problems. They were given other problems but their results were disregarded.

The additional problems which were presented at the end of the second period merely served to keep busy the students who had solved their problem in either or both of the first two periods. As these problems do not concern the experiment they need not be described.

(3) *Results.*

In Table V the success of the subjects in solving the control and the experimental problems is shown. Before the suggestions had been given the control and experimental problems were solved by approximately the same number of students. Thus 17.2 per cent. of the subjects solved the control problem and 17.8 per cent. solved the experimental problem. This means that at the outset the two problems were about equal in difficulty. However, in periods 3 and 4 the number of solutions to the

experimental problem is twice the number of solutions to the control problem. Thus in period 3, 18.6 per cent. of the remaining subjects solved the control problem, whereas in period 4, 37.4 per cent. of the remaining subjects solved the experimental problem. This difference in productivity in periods 3 and 4 seems to be primarily due to the lecture and suggestions which preceded period 4. The time consumed by the lecture can hardly be a determining factor because a rest period which permitted more time for contemplation on the problems preceded period 3. The lecture required attention on what was said as the students were required to take notes. They had been previously informed that they would be held responsible on examinations for the lecture.

Table V. *Solutions to control and experimental problems during each period.*

Period	Number of subjects attempting problem	Successes	Failures	Percentage which solved
Control problem.				
1	169	29	140	17.2
3	140	26	114	18.6
1 and 3	169	55	114	32.5
Experimental problem.				
2	169	30	139	17.8
4	139	52	87	37.4
2 and 4	169	82	87	48.5

Tables VI and VII give the results for men and women, respectively. Periods 1 and 3 were about equally productive for the men, the percentage which solved the control problem being 18.4 and 21.4 respectively. Period 4, however, was nearly twice as productive as period 2. The percentage of men solving the experimental problem during periods 2 and 4 was 22.3 and 41.3 respectively. In the case of the women, periods 1 and 3 were also about equally productive, the scores being 15.2 and 14.3 per cent. respectively. During period 2, 10.6 per cent. of the women

Table VI. *Solutions made by men.*

Period	Number of subjects attempting problem	Successes	Failures	Percentage which solved
Control problem.				
1	103	19	84	18.4
3	84	18	66	21.4
1 and 3	103	37	66	35.9
Experimental problem.				
2	103	23	80	22.3
4	80	33	47	41.3
2 and 4	103	56	47	54.4

Table VII. *Solutions made by women.*

Period	Number of subjects attempting problem	Successes	Failures	Percentage which solved
Control problem.				
1	66	10	56	15.2
3	56	8	48	14.3
1 and 3	66	18	48	27.3
Experimental problem.				
2	66	7	59	10.6
4	59	19	40	32.2
2 and 4	66	26	40	39.4

solved the experimental problem, but during period 4, 32.2 per cent. of the remaining ones solved the problem. Thus in the case of the women, period 4 was three times as productive as period 2.

In comparing the number of solutions appearing among men and women we again find the men more productive than the women. Of the men, 35.9 per cent. solved the control problem, and 54.4 per cent. the experimental problem. Of the women, 27.3 per cent. solved the control problem, and 39.4 per cent. the experimental problem. Thus, as in Exp. I, the men make higher scores than the women even though the women profit more by the suggestions. Without the suggestions the score made by the men is 46.2 per cent. higher than that made by the women.

V. DISCUSSION.

The results of both experiments indicate that when subjects are carefully instructed to guard against habitual activities and persistent directions, but to be on the alert for new points of view, there is a decided increase in reasoning ability as measured by the increase in solutions found to difficult problems. Taken together these experiments demonstrate conclusively that the increase in the number of solutions cannot be accounted for in any other way. If the suggestions given are responsible for the increase in the number of solutions, we have demonstrated that habitual behaviour and persistent directions which accompany problem-solving actually prevent solution-patterns from appearing. Thus a person may fail to solve a problem, not merely because of inability to form the solution-pattern, but because habitual activity actually inhibits the solution-pattern. One cannot equip a person with the ability to form solution-patterns, but one can train him to clear the ground so that the solution-pattern is not prevented from appearing. In other words, reasoning is, at least in part, the overcoming or inhibiting of habitual responses.

That reasoning is not only the inhibition of habitual responses has already been demonstrated by the writer in the case of the rat (6). When rats are presented with problems, the solutions of which are sometimes (1) opposed by a habit, and (2) not opposed by a habit, there are more solutions appearing in the latter case than the former, but the latter still presents them with difficulties.

The difference between men and women is of considerable interest. In Exp. I the problems used seemed to favour the men, and the superiority of the men over the women was marked. In Exp. II the score made by the men was 46.2 per cent. higher than that made by the women. In this case the problems used do not seem to favour the men. Whether the superiority of men over women in such problems is innate, or a function of differences in experience and interests cannot be decided from these data.

Billings (1) found a similar sex difference in college men and women. He found that men mastered the information required for problems 6.2 per cent. better than women, their I.Q. was 5.5 per cent. higher, but their ability to solve problems was 48.7 per cent. higher. The differences between men and women found by Billings and myself are very nearly equal despite the fact that the problems used were very different.

A further interesting sex difference is the fact that women profited more from suggestions than the men. This is difficult to explain, especially since Table IV shows that poor reasoners profit almost as much as good reasoners. Apparently the inferior score of the women is not entirely a matter of inferior ability.

VI. SUMMARY.

The purpose of the present experiment was to determine whether habitual activity actually inhibits the formation of solutions to problems or whether such activity merely comes in when there is nothing better to do. This was tested in two ways. (1) An experimental group was given general suggestions which consisted of advising students to try to rid themselves of persistent habits and directions in their thinking, and to maintain an attitude of receptiveness for new approaches to the problem. This group was compared with a control group in problem solving ability. (2) A different group of subjects was tested on its ability to solve a control and an experimental problem, the latter having been preceded by the above-mentioned suggestions.

The results are as follows:

(1) The experimental group of subjects made a score 24.1 per cent. higher than the control group.

(2) The experimental and control problems, which were found to be equal in difficulty before the suggestions were given, showed a marked difference in the number of subjects who solved them after the suggestions had been given. With the suggestions, the experimental problem was solved by twice as many individuals as was the control problem.

(3) Poor and good reasoners profited about equally by the suggestions.

(4) Women were markedly inferior to men in problem-solving ability.

(5) After the suggestions were given the scores of women rose much more than did the scores of men.

Because suggestions which advise against habitual and persisting activity increase problem-solving ability, it is concluded that reasoning involves the inhibition of persisting habits as well as the ability to form solution-patterns.

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(Manuscript received 7 October, 1932.)

THE AMERICAN *v.* THE GERMAN METHODS OF APPROACH TO THE STUDY OF TEMPERAMENT AND PERSONALITY¹.

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(*From the Psychological Laboratory, Cambridge.*)

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I. INTRODUCTION.

"The history of philosophy is to a great extent that of a certain clash of human temperaments."
William James, *Pragmatism*, p. 6. London, 1907.

"The manner in which animals learn has been much studied in recent years.... One may say broadly that all the animals that have been carefully observed have behaved so as to confirm the philosophy in which the observer believed before his observations began. Nay, more, they have all displayed the national characteristics of the observer. Animals studied by Americans rush about frantically, with an incredible display of hustle and pep, and at last achieve the desired result by chance. Animals observed by Germans sit still and think, and at last evolve the solution out of their inner consciousness."

Bertrand Russell, *An Outline of Philosophy*, pp. 32-3. London, 1927.

WHEN any insuperable difficulty seems to arise in any psychological or philosophical sphere, or when there is any vital disagreement between the theories and results of different investigators, then, according to the principle that is expressed in these two quotations, the discordance is due far less to objective complexities in the problem itself than to the

¹ This paper is intended as a theoretical introduction to a series of articles by the present writer⁽¹⁾.

subjective organization of the mentalities which have approached the problem. A useful illustration may be drawn from the mechanical sciences. A common situation, in that field, is a complicated, though balanced, system of forces, such as the structured framework of girders which supports a roof. Now when a new force is added to the system, as by putting a chimney on the roof, not only is the centre of gravity of the whole system displaced to a certain extent, but also the stresses and strains in every part of the system, *i.e.* in each girder, are somewhat altered. So in psychology, there always appears to be some *loading*, or extra emphasis of certain factors, which thereupon influences the whole organization or system. Other special inclinations appear among other investigators, with the result that the two or more systems tend to become mutually incompatible. The methods of research are determined by these 'loadings,' and the conclusions reached by the research confirm the one system and fail to confirm the other. The psychologist's own personality, his training and his particular interests and dispositions are unwittingly reflected in all that he does, and have to be taken into account in evaluating his contributions to psychology.

There is no need to multiply instances of this universal tendency in human thought. Bacon's description of 'Idols,' in *Novum Organum*, is still worthy of consideration. Bentham, in his *Theory of Fictions*, analysed the reification of language; and his work has been followed up by Ogden and Richards in *The Meaning of Meaning*, where it is shown that many of the concepts of natural science and philosophy are anthropomorphic names which have been hypostatized into self-subsistent entities. Freud's account of errors, and Bartlett's experiments on memory demonstrate the influence of affective complexes and sentiments on our cognitive functions. Spranger's elaborate system of 'values,' each with its own 'instinctive rationality,' Bergson's attack on the 'cinematographic' nature of intellect, and Whitehead's 'fallacy of misplaced concreteness,' are further descriptions of the same phenomena.

Naturally we are always ready to admit that the other side is biased. But the word bias has an ethical connotation which should be excluded from psychology¹. The so-called irrationality of the human mind may account for many futile disagreements among psychologists, but it is also essential for progress, as Boring has pointed out⁽³⁾. Divergencies among different investigators which lead to controversy, and the hypo-

¹ Lewin believes that contemporary psychology, when it contrasts the 'normal' with the 'pathological,' or the 'rational' with the 'irrational,' is on the same level of development as Aristotelian physics, with its 'heavenly' and 'earthly' forms of motion⁽²⁾.

statized 'fictions' contained in their theories, are the stepping stones to fuller knowledge. Moreover these 'loadings' are far more variegated and subtle than mere affective biases which influence individual minds. They include racial dispositions, cognitive categories and modes of thought common to the whole of a school, to a particular community, or to a generation. Another aspect which has been insufficiently realized is their two-sidedness; they always tend to become stereotyped and caricatured in the minds of those who oppose them. In other words, the 'Idol' is not only the product of the person who worships it, but it is also a man of straw, set up to be knocked down by disbelievers.

The contrast between American and German psychology, to which Russell has drawn attention in the paragraph quoted above, is an obvious illustration of these principles. The cleavage, in every branch of psychological research, has been manifest ever since Cattell undertook the investigation of individual differences in reaction times in the Leipzig laboratory, an experiment which Wundt called *ganz amerikanisch* (cf. (4) p. 319). The kind of problems upon which they concentrate are characteristic. Maller⁽⁵⁾ has shown, by analysing recent bibliographies, that less than 20 per cent. of the books and articles, at the present time, which deal with statistics, mental tests, and educational psychology are by Germans (presumably most of the remaining 80 per cent. are by Americans). Whereas in the fields of special mental conditions, sleep and dreams, sex behaviour, criminology, philosophical and psychiatric studies of character and personality, more than 50 per cent. are German. But even when representatives of the two countries study the same problems, *e.g.* animal intelligence, meaning, or the organization of personality, the methods of approach are so different that opposite conclusions are often reached.

It is hardly an exaggeration to say that 90 per cent. of the followers of the typical German schools regard American investigations and theories as futile, and that 90 per cent. of American psychologists return the compliment. According to Lehmann and Witty⁽⁶⁾, out of 204 recent American Ph.D. theses, only 24 per cent. made use of literature in any foreign language, as contrasted with 65 per cent. which employed statistical methods; and the relative discrepancy is increasing¹. The language barrier is not a sufficient excuse, since at many American Universities some knowledge of German is essential for the Ph.D., and most of the German literature is summarized in *Psychological Abstracts*. German

¹ It is also noteworthy that, in a recent American textbook which aims to cover the whole field of personality diagnosis, only 5 per cent. of a bibliography of nearly 1000 books and articles is German. Cf. Allport⁽⁷⁾.

psychologists, likewise, who generally possess a fair knowledge of English, have absorbed scarcely any of the literature that deals with developments in American research. They are still flogging the dead horse of Watsonian Behaviourism; and in the field of personality their knowledge of American methods and theories rarely seems to extend beyond 1914.

II. THE STUDY OF PERSONALITY.

It must be seldom that five minutes of a man's waking life can pass without his reacting in some way, during that time, to other men. Even if he does not see or hear members of his species, yet most of his thoughts concern them, or his explicit behaviour implies their influence. And in every such physical or mental contact between human beings there is found a feature which we may call—a judgment of personality. If one meets a friend or a stranger, one talks to him differently according as one judges him to be an optimist, a pessimist, an intellectual, or an athlete, etc. And one believes that the epithets (*i.e.* the personality trait-names) which one applies to people will continue to be applicable to them to-morrow, or next week. Köhler and Yerkes¹ have shown that apes possess distinctive and consistent personalities, and Pavlov⁽⁸⁾ has described temperamental differences among dogs. Art, law, education, magic and religion have concerned themselves with the study of personality for untold ages. Psychology, however, has only recently entered upon its 'proper study.' The double stimuli of psychopathology and mental testing were responsible for diverting it from its pursuit of the abstract, average, mind to the investigation of concrete, individual persons.

For the purposes of the present discussion there is no need to take up arms in the battle over definitions of personality (*cf.* (10) pp. 681–8), or of its innate foundations, *i.e.* temperament. Let us consider it, in its colloquial sense, as, "What kind of a person so-and-so is, what he is like." How, we must ask, should the scientist set about studying this phenomenon which everybody recognizes, but about which no two people agree?

All science, it seems, starts out from subjective experience which, as

¹ "Our observations convince us, as do Köhler's, that such terms as temperament, individuality, and personality are as useful, nay even as essential, in the description of the chimpanzee as of man....Anyone who doubts the existence of extremely different characteristics in individual chimpanzees need spend only a few hours before the cage of these animals to be convinced that they are quite as individual and quite as different in their traits of temperament and character, in their liability to and expression of tough and tender emotions as we are" (8, pp. 278 f.).

a result of controlled observation, attains a universal and externalized status. By means of intellectual inference and conceptual generalization, scientific laws are discovered which express invariable sequences within the phenomena that are observed. The aim of a scientific psychology of personality should be similar; it should try to analyse, systematize, measure and predict the activities of mankind, as physics has done with the activities of gases, metals, and the like. It should summarize definitively our experiences of human nature and the interactions of individual human beings. But its task is far more difficult than that of physics, for two main reasons: first, the phenomena of human nature are more complex than physical phenomena; secondly, our judgments of personality are much more fallible than our observations of objective events in the physical world. American and German psychologists have partially recognized these difficulties, but their reactions to them have been very different. Let us attempt first an analysis of the general trend in American studies.

III. THE AMERICAN PSYCHOMETRIC APPROACH TO PERSONALITY.

The American psychologist admits that, owing to the complexity of human organisms, the study of personality is unlikely ever to achieve the same exactness as physics and chemistry. Too many disturbing factors influence any single piece of behaviour of the individual for strictly objective experimentation or the establishment of invariant laws to be possible. The most that can be hoped for is to establish statistical tendencies, based on large numbers of observations, which may eventually cancel out the errors and deviations that are unavoidable in any single observation. The corollary of this statistical outlook is that such errors and deviations are ascribed to 'chance.' Thus mass investigation takes the place of detailed study of individual events and persons. In most sciences, precise measurement follows on the comprehensive observation and analysis of the phenomena in question, but the psychometrist seems to have jumped a stage and to have concentrated upon measurement before he knows what he is measuring. Thorndike's dictum is characteristic: "Whatever exists, exists in some amount. To measure it is simply to know its varying amounts"⁽¹¹⁾. By dealing with averages and probabilities the meaningfulness of the single concrete reaction or observation is neglected.

The American psychologist has also been greatly influenced by the subjectivity and bias that are apparent from his experiments with esti-

mates or ratings of human traits, with judgments of personality from external signs, etc. He has concluded that we usually see one another through a 'halo,' a general favourable or unfavourable impression¹. Our notions of a man's traits are frequently based on the most sketchy observations of his factual behaviour. We jump to conclusions and generalizations on quite inadequate grounds, failing to notice such characteristics as do not fit in with our stereotyped picture of him. And since no two observers see the man during the same phases of his existence, nor possess identical prejudices concerning him, it is seldom that these judges agree in their personal opinions.

Hence there has arisen the Behaviouristic tendency in the study and measurement of human aptitudes and personality traits. Traits, like instincts, are looked on with disfavour, and considered to be resurrected 'faculties.' The American psychologist usually denies that they are dynamic, existential entities within the organism, which determine behaviour. Rather are they constructs in the minds of the observers, who generalize and agglomerate under a few 'blanket' epithets the immense complexities and inconsistencies of behaviour. Sociability, for example, is not a general trait because people are found to vary in their sociability in different situations, and in the eyes of different observers. A child may be shy and reclusive at school, but frank and sociable at home. If several tests of this hypothetical trait are applied to a group of subjects, they seldom intercorrelate highly with one another, nor with ratings. According to the *doctrine of specificity*, which is widely accepted in America, the subject's reactions to a number of test situations are dictated, not by his general traits, but by innumerable specific, independently functioning habits of response. If, however, two or more tests do overlap in their results, this is ascribed to the presence of common identical objective elements in the test situations. This doctrine is already familiar in American educational psychology, where all transfer of training is denied. "What you do to the mind by means of education knows its place; it never spreads. You train what you train" (12), pp. 4 f.). The so-called objective or 'new-type' educational tests, which have largely

¹ Psychologically, the halo in judgments of personality is the exact counterpart of the loading in methods of investigation. It was shown, in the introduction, that psychological schools should not be regarded as springing from the irrationalities of human thought; similarly contemporary psychometrists are tending to give up the notion of ratings as partly true, partly prejudiced. Instead the term 'reputation' is coming into use, a term which is comparatively free from ethical connotations. The rating which *B* receives from *A* is *B*'s reputation in *A*'s eyes, a reputation which includes *A*'s own personality and outlook as an important constituent.

replaced the ordinary essay-form examinations, are a further symptom of the same outlook on mental organization.

Both aptitudes and traits, then, are practically defined in terms of habits of behaviour. A man's mechanical aptitude or his sociability consist essentially in his reactions to mechanical and social situations. And since these reactions differ in different contexts, the aptitude or trait is considered, quantitatively, as the number or proportion of habits of behaviour of the particular type that is called mechanical or sociable. A few people show a very high or very low proportion of social reactions in daily life, the majority show a moderate or median proportion. Hence it is that a trait of sociability which, in the view of the psychometrist, does not actually exist, can yet be treated as a quantitative continuum, varying in amount in different people and normally distributed.

American psychologists approach personality almost exclusively *via* the test. A test is a reaction to a standardized situation, which has been proved to be symptomatic of behaviour in general. In other words, its object is to predict this degree or frequency of mechanical or sociable, or other reactivities. Thus the experimenter who wishes to 'measure' the aptitude or trait first devises a number of situations in which the desired reactivity is likely to be manifested. One test situation will not suffice, since the subjects' reactions to it may be affected by uncontrollable disturbing factors and 'chance errors.' A battery of tests (ideally, an infinite number), covering a wide range of reactions, is more likely to obliterate such deviations in the long run, and to represent more adequately the field of behaviour which is summarized under the name mechanical aptitude, sociability, etc. Each test situation is standardized, and the recording or scoring of the responses is made foolproof. Though the psychometrist does not realize the fact, yet this procedure implicitly involves two quite unjustifiable assumptions: first, that identical objective situations will produce identical subjective situations or meanings in the minds of all the subjects; and secondly that when two or more subjects give the same objective response, then that response is determined by identical mental processes within these subjects. The Behaviouristic experimenter tries to eliminate the influence of the psychological organization of his subjects upon the stimulus-response sequence simply by neglecting it. In actual practice, of course, the most inveterate psychometrist would deviate from strictly uniform conditions in individual Binet testing, in order to ensure a more favourable subjective situation. But the greater portion of psychometric investigation is carried

out by means of group tests, which set out to be entirely impersonal, and in which all departures from objectivity are barred.

The subject's aggregate frequency of response or score on the summed test situations is considered to be a measure of the amount of his hypothetical aptitude or trait. But satisfactory units are even more difficult to achieve in this field than in the attempted measurement of sensation intensities. For personality contains no homogeneous, uni-dimensional and discrete variables analogous to the variables with which the physical sciences deal. Owing to this lack of absolute, scientific units, the commonest method is to adopt some derivative of rank order or percentile units. That is, the test is applied to a large, presumably unselected, sample of the population, and a subject's score is then his standing relative to the norms and to the range of responses among that population. This very indirect method of scaling means that the psychometrist can study the personality of the individual only through the reactions of the group. Moreover the individual's total concrete response to the test situation, together with the complexities of the motives that led to his response, cannot be expressed quantitatively. Instead the experimenter is forced "...to select some specific aspect of behaviour on which all subjects can be quantitatively compared. This procedure results in the neglect of innumerable factors, different in each individual, which determine the particular bit of behaviour under examination" (13, p. 48). Psychometrics, in other words, cannot deal with differences between individuals, but only with differences between performances in tests. The patterning or organized form-quality of a single personality is inaccessible to the ordinary statistical techniques, and its very existence is denied by the doctrine of specificity simply because these techniques cannot take it into account. As Fox has stated: "Testers have lost their mental balance and are confusing administration with psychological procedure, so that devices invented for rapidly marking large numbers of answers have been erected into laws of the working of the mind" (14, p. ix).

All scruples, however, are allayed by the final process in the construction of the test, namely its validation by the ubiquitous correlational methods. The justification claimed by psychometrics is that it works, that tests of intelligence, for example, yield moderate correlations with scholastic grades, or that tests of sociability give even more moderate correlations with estimates of the subject's standings on this trait¹. Since perfect agreement is never achieved, the exceptions are again

¹ There is no need here to press the point that validity coefficients of about +0.40 to +0.70 actually represent a predictive efficiency of only 10 to 30 per cent.

ascribed to uncontrollable errors of measurement and observation, which may or may not be reduced by modification of the test situations and by further standardization of procedure. It is generally forgotten that the quantitative success of a test is due, not only to the scientific controls which have been applied, but also to the initial subjective 'hunch' upon which the test was based. Purely Behaviouristic methods and intellectual inference could never formulate the connection between tests of intelligence and the subsequent intellectual behaviour which the tests are designed to predict, had not uncontrolled introspection and 'intuition' done so beforehand. "Sensible investigators," says Keynes, "only apply the correlation coefficient to...confirm conclusions at which they have arrived on other grounds" (15), p. 426).

IV. PSYCHOMETRICS AND SCIENCE.

It is a highly significant fact that this "era of measurement," as Roback (16) calls it, has been confined so largely to America. It suggests that the movement towards the investigation of personality by means of tests and statistics must be strongly 'loaded' in some way, otherwise it would hardly have been so completely ignored in this country and in France, as well as in Germany. The American prefers to describe this movement as the study of individual differences, and he often considers it superior to ordinary experimental psychology, since the latter vainly attempts to systematize the mental process of the *average man*, while the psychometric approach makes capital out of the non-existence of this average man. However, there seems to be no parallel for such a development in the natural sciences (with the possible exception of genetics), and it is questionable whether the testing movement has not forfeited its scientific birthright by adopting this approach. Instead of trying to reach the ideal of achieving laws which express universal relations between the variables that are under observation or experiment, it has followed what appear to be the methods of technology or medicine. Physical laws, says Lewin (2), are independent of conditions of time and place ('historico-geographic' factors), and are in no way bound up with the concept of frequency of occurrence. Indeed, the law which governs the falling of bodies in a vacuum never occurs in our ordinary experience. Statistical psychology, on the other hand, considers lawfulness as a correlation coefficient which approaches ± 1.0 , under particular historical and geographic conditions. Individual events are determined by chance factors, and are therefore non-lawful.

According to Yule(17, 18), statistical methods, especially those which depend on the normal or Gaussian frequency distribution curve, are the scientific psychologist's confession of failure; they have developed because of the very imperfections of psychological experimentation. And Boring(19) shows that the normal form of distribution only arises when the factors which influence the variable are so numerous and so poorly controlled that they have to be commingled under the name of 'chance.' Is there any reason, he enquires, other than ignorance, why it should apply to mental data? It would seem to be fundamentally antithetical to the notion of a systematic psychology of personality. If the artist, the psycho-analyst, even the man in the street, do not usually invoke chance in their studies of personality, it is difficult to see why the psychologist should be interminably wedded to it. Myers(20) also suggests that the job of the psychologist is to investigate those very 'errors' which the statistician conceals behind the normal curve.

American investigations of personality appear to be analogous, not to the experiments of the biologist and physicist, but to the Wassermann reaction of the blood, and to tests of the breaking point of steel. Both of these were originated for practical, non-scientific ends. Similarly, therefore, psychological testing, which may find some justification in the difficulty of applying purely scientific experimental methods to personality, is probably motivated primarily by economic, educational and therapeutic ends. Possibly another factor which has contributed to mass investigation is the existence of large numbers of pupils and students in American schools, colleges, etc.; this necessitates the construction of group tests and so tends to minimize the study of the individual. But this is obviously a mere symptom of some more fundamental characteristic of American psychology and education. If one adopted the conventional European opinion of America, one would consider the testing approach as arising primarily from the desire to exploit and improve people, rather than from the desire to study people. Such an opinion, however, is probably a 'stereotype' in the minds of those who hold it, and not only a 'loading' in the actions of those to whom it is applied. Thus it accords better with the principles outlined in the introduction to this paper to conclude that the national philosophy which has produced pragmatism, Behaviourism and scientific management, has also produced the testing movement and the accompanying theory of specificity.

It is not denied for a moment that many of the chief investigators who have used tests have applied them as research instruments rather than for practical purposes. Terman(21, 22) quite rightly stresses this point.

Much important work, such as that on the relative effects of heredity and environment on intelligence, the two-factor theory, the halo effect, Terman's own studies of genius, and even the doctrine of specificity, is based on disinterested scientific impulses. What we should enquire, however, is if methods whose origin is technological are really suitable for scientific research purposes, or if these investigators have been beguiled by their own instruments.

According to Terman, practical motives have acted as a most valuable stimulus to science. Pasteur initiated the whole science of bacteriology while working on practical problems such as fermentation, rabies, and the diseases of silkworms. Many similar examples could be cited. But when examined more closely they appear to contradict Terman's claim, namely that the test is a scientific method. For they show that suitable practical tests and methods were achieved through a previous truly scientific study of the physico-chemical or biological nature of the problem. Tests of the breaking point of steel may have afforded us much scientific data on metallic crystal structure; but it would be more correct to say that scientific study of metallic crystal structure has enabled engineers to develop better tests of the breaking point of steel. The same holds good for medical tests of the blood. It is very doubtful whether any scientific advance has followed from strictly technological or therapeutic work, in spite of the fact that technology and therapeutics have stimulated or financed a huge volume of research which, by using scientific methods, has led to scientific as well as to practical advances. Similarly the psychology of personality is too often confused with religious education and mental hygiene, and with vocational adjustment or personnel work. All of these applications of psychology may have a considerable value of their own, but they ought to be the followers and not the leaders of the 'pure' science. No one would deny that psychological tests have been useful and will in the future be even more useful, yet surely it would be worth the psychometrist's while to find out first what it is he is testing. The preliminary study of fundamental theoretical problems seems to have been neglected for the sake of practical results (cf. (23)), or to have been left to psychiatrists, to artists and to German philosophers. And the testing method itself, primarily a means to a non-scientific goal, should not be hypostatized as though it was the only and exclusive approach to the truth about personality. On the contrary there is every reason to expect that the results obtained by the sole use of this method are unwittingly loaded. The exigencies of technology are by no means the same thing as the physicist's universal laws.

V. THE GERMAN QUALITATIVE APPROACH TO PERSONALITY.

Although German industrial psychologists are working along somewhat the same lines as American, the psychometric approach to personality is almost wholly uncomprehended in Germany. Many experimentalists, to be sure, employ rudimentary statistical methods, such as medians, rank order correlations, and the like, in working up their results. But they usually omit any mention of probable errors, reliability coefficients, etc. The simple range of scores is the only measure of dispersion that is at all frequently presented. Inexact tabular methods of expressing their findings are generally preferred to strictly controlled numerical formulation and algebraical treatment. Correlation coefficients are often derived from very small numbers of cases, and the degree of heterogeneity of the groups of subjects (a factor which has an enormous influence upon the size of the coefficient (cf. (24))) is not indicated. Far-reaching theoretical generalizations are often based on quite unreliable differences in the average scores of groups.

This is but natural, however, for the German experimentalist does not think statistically. He certainly does not consider that imperfect correlations are largely due to chance errors of measurement. To pile up large masses of numerical records does not appeal to him, because mere frequency of occurrence has never impressed him as a criterion of the truth. While recognizing, as does the American, the complexity of human nature, his reaction to this fact is to consider every piece of behaviour of an individual subject as meaningfully related to the subject's total personality. A test response—for tests of a kind are frequently applied in Germany—does not consist of so much trait or aptitude and so much uncontrollable error. Instead it is the expression of a wide variety of psychological factors which must therefore all be taken into account in determining the significance of this response. According to Gross, "Every individual quality is merely a symptom of a whole nature, can be explained only by the whole complex" (25, p. 61). Stern's *Personalistik* (26), Klages' *Ausdruckslehre* (27), and Spranger's 'cultural' psychology (28), are the chief deductive systems, which stress this consideration of every act as symptomatic of the total Person.

Take, for example, the responses to Rorschach's inkblot test (29) which are determined by the colours of the blots. The author of the test claims that these responses represent impulsiveness and highly toned affectivity in the subject. In order to validate this claim, the psychometrist would obtain the total colour scores of a large group of subjects and correlate

them with other criteria (e.g. ratings) of impulsiveness, and would very likely find a negligible or even a negative coefficient. But this result would not disconcert the proponents of the test. For the actual interpretation placed on these colour responses varies greatly with all the other types of response that the individual gives to the blots. Most certainly one subject who obtained twice as many colour answers as another would not be considered to be twice as impulsive, or even to possess the same trait as the other but in a different amount. And impulsiveness is not, for the German experimenter, a uni-dimensional and isolated trait, nor a quantitatively definable set of habits of behaviour. It is a quality of reaction which is dependent on and modified by the particular individual's special type of intelligence, by his interests and values, and by his fundamental affective attitude or biotype. The correlation would therefore be accepted as low, not because the colour answers have poor diagnostic validity, but because each subject's affectivity is unique, and can only be understood in the light of the total form-quality of his make-up. Indeed the validity is implicitly assumed to be perfect, so long as the experimenter possesses sufficient skill in interpreting the data that are derived from the test as a whole.

Here is a difficulty that might seem to be insuperable. The psychometrist may justifiably ask, what is the use of a test which cannot be validated by the scientific, quantitative methods which he has been brought up to believe in? And he would say that many of the correspondences claimed by Klages, Rorschach, Jaensch, etc., between test responses and psychological traits are little better than homoeopathic magic. For example, upright, vertical handwriting is associated by Klages with stability and self-control; exaggerated size, pressure and speed of writing with explosiveness of temperament, and so on. Empirical evidence of such correspondences is hard to find. Possibly Klages did notice these features in the handwritings of a few subjects whom he believed to be characterized by these traits. But as he had already decided on this interpretation through deductive reasoning and 'intuition,' he was, perhaps, 'looking for' the traits and therefore found them to his own satisfaction. Another German psychologist who preferred a different theory of the organization of personality might easily have claimed, on equally good grounds, that the features represented some entirely different trait. If the intuition of the experimentalist is allowed to determine the symptomatic meaning of an act, who is to decide which interpretation is the best one? The validation of German diagnostic methods is often unashamedly subjective. For example, the experi-

menter procures case-studies of his subjects, either from school-teachers or psychiatrists; or sometimes he even writes these studies himself. He then discusses in detail the results of the test and tries to show verbally the agreement with the case-studies. These *ex post facto* correspondences are, of course, quite unconvincing to the psychometrist.

At the same time the lack of validity which is obtained when correlational techniques are applied to German methods is very likely due to the artificiality and unsuitability of such techniques for expressing the complex unities or patternings within the individual. In the field of graphology, for instance, some Germans have developed methods of comparing the total script with the total personality, and their diagnostic findings are extremely favourable. The American, on the other hand, makes detailed measurements of minute features of the scripts of a large group of subjects, correlates these measurements with estimates of the subjects' traits, and concludes that his zero coefficients have 'debunked' graphology once and for all (cf. (13), Chap. ix).

A good summary of the present position is contained in the following quotation from Thurstone ((30), p. iv):

...It is only fair to say that personally I do not believe that these correlational methods and particularly the reliability formulae have been responsible for much that can be called fundamental, important, or significant in psychology. On the contrary, the correlational methods have probably stifled scientific imagination as often as they have been of service. As tools in their proper place they are useful but as the central theme of mental measurement they are rather sterile. On the other hand I should never recommend the loose thinking that is current in some European psychology where biometric logic is conspicuous by its absence. What is needed there is more of the biometric methods for testing clever ideas.

VI. TYPE PSYCHOLOGY.

Since the German psychologist makes little use of averages, standard deviations and correlations in generalizing from his experimental data, some alternative procedure is clearly necessary. This is supplied by the type. But psychological types, as employed by Jung, Kretschmer, Rorschach, Spranger, Jaensch, and others, are entirely foreign to American conceptions. They are logical generalizations, rather than empirical groupings. The psychometrist would criticize Kretschmer's typological theory, for example, along the following lines. Bodily physique, skin texture, cycloid *v.* schizoid tendencies, social-asocial traits and other mental qualities are each of them distributed in the total population according to the normal or Gaussian curve. A few persons

manifest extremely high or low standings on any one characteristic, but the majority are near the average. Moreover, no two characteristics intercorrelate at all highly, so that it is entirely fallacious to think of some persons as belonging to one (pyknic) type and possessing all of one set of characteristics, other persons belonging to another (asthenic) type and possessing the opposite set of characteristics. In fact the typologist seems to go to the other extreme from the psychometrist, since he neglects all empirical individual differences and only allows the existence of these hypostatized subjective differences.

But this criticism is again a misunderstanding of the German approach. Psychological characteristics, from the qualitative standpoint, do not imply a normal distribution in a random population, as they do to the psychometrist. Hence the German is not at all impressed by the argument that bimodal distributions practically never occur in nature. Indeed few but E. Jaensch ever take any notice of such an argument. Moreover, frequency of occurrence has nothing to do with the type concept. According to Klüver (31, 32), the postulation of a type implies that personalities are viewed with respect to their "essential characteristics." A type reduces a set of phenomena (such as bodily build, psychomotility, psychoneurotic tendencies, etc.) to "one fundamental formula." The type is a generalization made by the typologist of the way in which these phenomena appear to him to interact dynamically; it is not a mere collection of statistically associated objective phenomena. It is openly admitted that no single individual is ever likely to exhibit all the *typical* characteristics within his own psyche. Naturally there are wide variations between the doctrines of different typologists; the data which are cited by the present writer in Part II of his account of the Rorschach test (29) exhibit an immense complexity and overlapping among the various schemes. Yet all possess the common feature of serving to make intelligible (at any rate to their authors) the formalities of personalities, by means of the abstraction and interpretation of the main dispositions and traits as a hypothetical dynamic system or totality. Jung's introvert and extravert types are based, as Klüver shows, on "the long experience of a physician...made accessible to us in condensed form." And they are certainly not regarded as empirically established aggregates of isolated functions, attitudes, or habits of behaviour, which are bimodally distributed in an unselected population.

VII. THE ENGLISH APPROACH TO PERSONALITY.

One might imagine that English psychologists would be especially well fitted for investigations of personality which would combine the advantages of, or avoid the disadvantages of both the American and the German approaches. Yet it must be admitted that important English work in the field is conspicuous by its absence. Certainly our intelligence testing is well advanced, though, as far as techniques go, we have done little but follow French and American leads. And our industrial psychology may well claim to have forged ahead of that of any other country, in many respects. If our vocational guidance is particularly successful, it is probably due to the development of methods which temper American objectivity with the more artistic and clinical outlook of German psychotechnology (33). But apart from applied psychology our contributions to the experimental study of temperament and personality are meagre.

This is all the more curious when we recollect that Francis Galton was certainly one of the main founders of the movement. If the amount of influence a psychologist exerts upon the progress of psychology all over the world (not only in his own country) is any criterion of greatness, then Galton is probably our greatest psychologist born within the past hundred years. Few of us, perhaps, would care to admit this estimate, presumably because we are out of sympathy with his work, which others have eagerly absorbed.

In the present century, two contributions stand out—Webb's elaborate study with ratings in 1915 (34), and Burt's development of the method of estimating emotional qualities from interviews and performance test situations (35, 36). The extreme importance of halo effects and subjective prejudice in ratings had not been discovered when the former research was carried out, so that its conclusions are now somewhat discredited; but it was a most valuable pioneer attempt. Burt's work was also essentially subjective; the validity of his method has never been demonstrated. And yet the fact remains that it is almost the only method which is of real practical value on the temperamental side of research in vocational guidance and juvenile delinquency. And more adequate consideration has been devoted to its theoretical background than to that of any of the well-known 'objective' psychodiagnostic methods.

Most of the remaining research in this country has concentrated on insignificant motor performances, such as the Downey-Will-Temperament tests, and perseveration. These are far too elementary and peripheral to show consistent and valid connections with accepted traits of personality.

Sensory-motor tests, at a similar psychophysiological level, were widely exploited towards the end of last century as possible measures of intelligence; but they did not make any call upon the subjects' intellectual functions, and therefore failed utterly in their purpose. Since they make no call upon the affective functions, and have very little *meaning* for the subjects, they appear, according to the consensus of critical research, to be unrelated to personality also. The same holds good for the psychogalvanic reflex. Practically no claim has ever been made for its affective or conative significance by any one experimenter which has not been directly contradicted by the findings of some other experimenter. Those who work with these physiological and sensory-motor functions still seem to be at the stage of confusing 'equivocance' with 'equivalence' (cf. (37)). (American psychometrists are rapidly emerging from this fallacy, but the Germans are still immersed in it.) For example, fluctuations in cancellation speed or in quickness of reaction time may conveniently be given the *name*—'oscillation' or 'variability.' But it certainly does not follow that measures of these fluctuations in an unselected group of subjects bear any significant relation to the 'instability' or 'variability' of the subjects' temperamental make-up (cf. (13), pp. 123-8). It is unfortunate that so much of our energy is being wasted in this field, when far more vital problems are being followed up in Germany and in America. We should be able to avoid the 'loadings' which tend to distort so much of the work in these two countries, and to develop a scientific psychology of personality more successfully than either.

VIII. POSSIBLE DEVELOPMENTS IN THE EXPERIMENTAL PSYCHOLOGY OF PERSONALITY.

Only the most tentative outline of some of the possibilities of reconciliation will be attempted here. Qualitative clinical methods and quantitative objective standards should not necessarily be incompatible. Controlled empirical techniques might be developed which would aim at validating manifestations of personality as concrete *Gestalten*, rather than as abstracted statistical ciphers. On the one hand the complexities of human nature and the meaningfulness of the unique individual reaction should not be forced into unsuitable quantitative moulds, and on the other hand means should be devised for overcoming the subjectivity and fallibility of uncontrolled intuitive and clinical procedure.

One method which fulfils these conditions is that of matching; it should have a very wide applicability. For example, half-a-dozen specimens of handwriting are presented to a group of judges together with

photographs of the subjects or writers. The judges have to match or guess which script belongs to which face. Gramophonic reproductions of the subjects' voices, the shape of their hands, cinematographic records of their walk or gait, their professional occupations, examples of their literary or artistic productions, short case-studies, and many other such expressions of personality have been matched or compared together by various investigators¹. When the statistical implications of the method have been worked out more fully, it may be employed for correlating any single aspect of personality with any other. Hence it may serve to throw light on fundamental problems of human organization.

Perhaps the most pressing problem in the field is the significance of each 'level of expression' for the person as a whole, and the interrelations of these levels. The lowest level is the anatomical, including constitutional types, measurements of the features, physique, etc. Have these objective data any significant connection with the higher social and emotional traits? Endocrinological, chemical and psychogalvanic measurements constitute a second level. Motor performances, perseveration, variability in reaction time and the like, should next be considered. Probably spontaneous movements are more important—handwriting, gestures, gait, dynamics of the voice and *Mitbewegungen*. Perceptual and imaginal qualities are frequently studied in Germany, but their true relationship to personality is unknown; distinctive types have been alleged to occur in reading and tachistoscopic experiments, in the description of objects and recall, in interpretations of inkblots. Are literary and artistic style, speech, dress, house furnishings, tastes in music, reading and recreations as characteristic of the person as we generally suppose? Again, the connections claimed by psycho-analysts between childhood fixations and subsequent character traits should be established (or disproved) by more controlled methods. The judgments of first acquaintances and of intimate friends should be examined in the light of the personalities of the judges themselves. The trustworthiness of the subjects' own introspections, in their diaries, or when questioned, needs fuller analysis. The significance of American objective test records, and of free association, interest blank and Pressey X-O responses is still dubious.

Obviously the main difficulty is that of the criterion against which these levels of expression may be compared. Probably the aid of psychiatrists and clinical psychologists should be enlisted. They should draw up detailed case-studies of the subjects, based as far as possible on facts of their life histories, and connected up with hereditary and

¹ For examples and criticisms of the method, cf. (12), pp. 11-14, 197-203.

environmental influences. A wide variety of normal persons and abnormal patients should be included among these subjects. The significance of many of the above manifestations will only be revealed by genetic study and interpretation of individual personalities. Yet objective controls need not be wholly abrogated; the 'longitudinal' approach of the Toronto investigators (38, 39), and V. V. Anderson's (40) work in a department store are instances of a synthesis of psychiatry with psychometrics.

The statistical, 'horizontal' approach (or as Lewin (2) would call it—the 'phenotypic' approach) is much more limited, in spite of its superior scientific standing. Probably its main function is to confirm conclusions obtained by other means; it is doubtful whether it contains in itself any possibilities of progress. At the present moment statistical methods in personality are wedded to tests, and to the search for practically useful results. But the investigator of personality would do well to ignore the practical applications of his work for the time being, and to discard the test altogether. For a subject's score on a test represents simply a sample of his behaviour, which may or may not possess predictive value for future behaviour. If this sample is taken from too elementary a level of expression, then it probably measures nothing beyond itself. If it is taken from a higher level, it usually has to be abstracted from its meaningful context, or subjected to artificial conditions, in order to be turned into a quantitative cipher. The experimentalist should therefore try to record samples of behaviour under the natural conditions of real life.

Here Burt's method, mentioned above, is a valuable starting-point. But it could be developed in several respects. First, more elaborate situations, such as those devised by Henning (41) in his *Zweipersonen Experimente*, should be arranged in order to elicit more significant responses. Secondly, greater objectivity might be achieved by adopting the admirable observational techniques of Goodenough (42), Thomas (43); and others. Thirdly, follow-up studies should be instituted for determining the validity of the experimenter's predictions and judgments. Fourthly, the method should be extended for use on adults, also on animals (whose personalities have seldom been systematically studied).

In the list of English contributions (Section VII), Spearman's statistical techniques were omitted because their applicability to the complex affective functions of personality has not yet been demonstrated (cf. (13), Chaps. II and V). But they would be of great value in the field of opinions, attitudes and interests. The organization of peoples' attitudes to socialism, nationalism, religion, morals, art and science, might well be

examined through factorial analysis. More important still would be a statistical study of the biosocial aspects of traits and personality types. For example, Webb's investigation with ratings and Cattell's recent extension of it(44) should be regarded as studies, not of the traits of the people who were rated, but of the ideas of the traits in the minds of the raters, and of the various interconnections between these ideas. Personality is double-sided; it depends upon the behaviour of the persons who are observed, and upon interpretations of this behaviour by their observers. Both aspects require exploration.

This sketch of future possibilities is necessarily very incomplete. For instance, no mention has been made of the storehouse of generalizations about personality contained in proverbs, folklore and literature. Apart from the matching method, the concept of levels of expression, and the factor analysis of opinion, we have dealt mainly with the investigation of individual differences. We should not forget that the latter is not an end, but only a means to the establishment of general laws about human nature.

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(Manuscript received 7 April, 1933.)

THE MERRILL-PALMER SCALE OF INTELLIGENCE TESTS FOR PRE-SCHOOL CHILDREN APPLIED TO LOW-GRADE MENTAL DEFECTIVES¹.

By R. G. GORDON.

- I. *Introduction* (pp. 178-80).
- II. *The Merrill-Palmer scale of tests* (pp. 180-2).
- III. *The findings of the application of the Merrill-Palmer scale to low-grade mental defectives, and its correlation with the Terman and Porteus Tests* (pp. 182-4).
- IV. *The performance of mental defectives in the separate groups of the Merrill-Palmer tests* (pp. 184-6).
- References* (p. 186).

I. INTRODUCTION.

HERETOFORE the class of mental defectives who are in this country included in the group of idiots and imbeciles have not been studied to any extent from the psychological point of view. It may be argued from the administrative standpoint that there is no particular point in such an investigation since these low-grade cases must in any case be confined in institutions and can never hope to fend for themselves in the community. Nevertheless, it does seem possible that if some series of tests could be found whereby such cases could be grouped and classified, light might be thrown on certain problems. For example, do the idiots and imbeciles form an intermediate series between the intelligence of the chimpanzee and that of the feeble-minded who lead up to the normal? Is there a definite correspondence between the general intelligence and abilities of these low-grade idiots and imbeciles and those of the young pre-school child, i.e. can they do the same sort of tests in the same sort of way? The answers to these questions are of importance from several points of view. It is sometimes suggested that the low-grade defective is a being apart from the rest of the human race and almost represents a different species. But against this, it is reasonably well established that while the brains of the idiot and imbecile class are microscopically distinguishable

¹ This investigation was carried out as part of the research activities at Stoke Park Colony under the general direction of Professor R. J. A. Berry.

from those of the anthropoids on the one hand and from the normal man on the other, this difference is rather one of quantity than of quality. That is to say, that though the number and complexity of neurones in the imbecile brain is greater than that in the chimpanzee brain, but less than that in the adult human brain, the general architectural arrangement of all three is definitely comparable. There is less convincing evidence that there is a corresponding though less marked difference between the brain of the feeble-minded and that of the normal adult, and some refuse to admit that there is any such difference. While it may be agreed that the direct evidence is less complete for the very good reason that the slighter and more subtle differences are more difficult to demonstrate, there is a certain amount of this direct evidence and a great deal of indirect evidence. Anatomically, therefore, the series from anthropoid to normal human brain does not appear to be broken. If it can be shown that the mental abilities of the low-grade cases show a definite unbroken chain with those of the feeble-minded, dull and backward, and normal, it is justifiable to infer that there is no essential difference either in brain or mind between the various grades of defectives; and also it might be possible to demonstrate by further work that these low-grade defectives constitute a psychological missing link between the mentality of the great apes and that of the normal human being.

The other problem which requires a good deal of further investigation is the difference of behaviour between defectives and children of corresponding mental age. If the general intelligence and mental abilities are similar, it is legitimate to infer that the differences of behaviour are due to differences of physical rather than of mental growth. The greatest discrepancy of physical development will naturally be found in the low-grade cases, and at the same time, the behaviour being simpler will be easier to study and to assess and compare with that of the normal child. It is hoped later to study this subject, but before this can be done, it is necessary to discover whether the abilities of these low-grade cases really correspond to those of the pre-school child. It is the purpose of the present investigation to throw some light on this problem.

The Binet tests or their modifications are now fairly generally accepted as being a reliable guide to the abilities both of the normal child and of the feeble-minded, and from a study of very numerous cases of both it is found that the feeble-minded abilities and methods of approaching problems do correspond to those of children of corresponding mental age.

Although the Terman scale begins for the age of 3, it is the opinion of most investigators that it is not specially suited for the pre-school child,

and it has also been shown that it is not easily applied to the imbecile and idiot class. So far as the pre-school child is concerned, the battery of tests which seems most satisfactory and has been most successfully standardized is the Merrill-Palmer scale. The present paper deals with the application by Miss M. Havers of this scale to 254 mental defectives of the imbecile and idiot class. The scale starts with tests suitable for the mental age of $1\frac{1}{2}$ years (18 months) and ranges to those suitable for the mental age of $6\frac{1}{2}$ years (78 months). It was considered that the last 12 months, *i.e.* 66–78 months, came well within the limit of reliability of the Terman revision of the Binet scale, which is used at Stoke Park Colony for the higher-grade defectives along with the Porteus maze tests. Fifty-four cases who came out on the Merrill-Palmer scale between 66 and 78 months were tested by Misses West and Bergman on the Terman and Porteus scales and the results correlated, to determine whether the results of the Merrill-Palmer tests could be properly compared with those of the Terman-Porteus tests.

II. THE MERRILL-PALMER SCALE OF TESTS.

It is neither possible nor necessary to give a detailed description of this battery of tests here, since this can be found in full in Dr Rachel Stutsman's book *Mental Measurement of Pre-school Children*. Suffice it to say that the battery was compiled after trying out 79 tests proposed by various authorities, of which 38 were selected as being specially suitable for use with the child of pre-school age. These were then standardized with a large number of children in the Merrill-Palmer school. It is the object of all intelligence tests to present a scattered group of tests for various abilities so that inequalities in the individual may be evened out over the whole battery of tests. So far as individual testing is concerned, this is of course desirable and indeed necessary, but when considering a group it makes it difficult to pick out that group's ability in any given direction, and this may become practically impossible with such a complex battery as that of the Terman scale. In the Merrill-Palmer scale, however, while the principle of scattering is maintained, the relative simplicity of the tests makes it possible to group them more easily, and they in fact divide themselves into four sections:

- (1) All or none tests—obeying simple commands.
- (2) Language or action agent tests.
- (3) Tests of motor co-ordination.
- (4) Form boards and picture tests.

The battery consists of 93 tests arranged from 1 to 93 in order of difficulty. We find that the first group—obeying simple commands—is the easiest for the standard normal pre-school child. This is computed as follows:

There are 18 tests of this group and we find that these are placed in the total scale as follows:

1, 2, 3, 6, 7, 10, 11, 13, 17, 20, 23, 34, 35, 37, 54, 56, 82, 87.

If these numbers are added together and divided by 18 we get an arithmetical mean. This figure for group 1 is 28.8; for group 2 in which there are 12 tests it is 38.7; for group 3 in which there are 33 tests 46.2; and for group 4 in which there are 30 tests 63.6. For normal pre-school children, therefore, the relative expectation of difficulty of the four groups of tests is roughly (for the numbers of tests are too small to make an exact comparison) 28.8, 38.7, 46.2, 63.6, taking 1 as the easiest test and 93 as the most difficult. How the defectives compare in this respect will be seen below.

Group 1. The all or none tests—obeying of direct commands.

These consist of simple commands such as: Move this box from the table and put it on the chair; Throw me this ball; Cross your feet; Stand on one foot: up to more difficult ones, such as: Copying a circle, copying a cross and copying a star. Each test demands a simple performance which involves a certain amount of spatial discrimination and manipulation, but a minimal use of language symbols.

Group 2. The language or action agent tests.

These involve the attachment of meaning to words and recognition of words. Thus questions are asked, as for example: What bites? What boils? What roars? etc. Then more difficult questions, such as: What does a kittie say? What is your name? What is this (chair) for? Finally repetition of words and word groups.

Group 3. Tests of motor co-ordination.

These consist in fitting pegs into holes—fitting cubes into a box—doing up buttons—building a tower of graduated blocks—building pyramids of blocks, etc.

They involve a certain amount of spatial manipulation and discrimination combined with some degree of motor co-ordination, that is to say, discrimination and integration of movements which involves activity at a cortical level.

Group 4. Form boards and picture tests.

These consist in setting cut-out shapes or pictures into corresponding cavities in a board (Seguin Form Board, Mare and Foal test); or building up parts into a whole (Manikin test, picture puzzles); or matching pictures of simple objects (Decroly matching game). They involve spatial discrimination and manipulation of a more complex form and some motor co-ordination.

III. THE FINDINGS OF THE APPLICATION OF THE MERRILL-PALMER SCALE TO LOW-GRADE MENTAL DEFECTIVES AND ITS CORRELATION WITH THE TERMAN AND PORTEUS TESTS.

254 mental defectives of both sexes and all ages over 8 (*i.e.* definitely above the maximum mental age for which the scale was devised) of the imbecile and idiot grades, were subjected to the Merrill-Palmer scale of tests. The following table represents the scoring. All those over 16 were counted as adults.

Age	18-30 months		30-42 months		42-54 months		54-66 months		66-78 months		Totals		
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	
Adult	3	1	15	23	11	9	16	14	25	46	70	93	163
15-16	—	—	1	3	—	—	—	1	1	1	2	5	7
14-15	—	1	—	4	—	2	—	3	2	2	2	12	14
13-14	—	—	—	2	—	1	—	2	—	2	—	7	7
12-13	2	—	2	1	1	2	2	2	2	3	9	8	17
11-12	—	—	3	3	2	1	1	—	2	5	8	9	17
10-11	—	1	3	2	1	2	—	—	1	5	5	10	15
9-10	1	—	1	—	—	—	1	—	2	—	5	—	5
8-9	1	—	3	3	1	—	—	—	1	—	6	3	9
Totals	7	3	28	41	16	17	20	22	36	64	107	147	254

It may be mentioned that these cases were in no way specially selected, but were simply the low-grade cases as they occurred in the course of routine testing. It will therefore be seen that they come well within the limits of the scale, the majority ranging towards the higher limits of the tests.

Fifty-four cases which tested out at 66-78 months on the Merrill-Palmer scale were also tested on the Terman revision of the Binet tests and on the Porteus tests.

Correlation coefficients were worked out between:

- (1) The Merrill-Palmer scores and the Terman scores.
- (2) The Merrill-Palmer scores and the Porteus scores.
- (3) The Merrill-Palmer scores and the combined Terman-Porteus scores.
- (4) The Terman scores and the Porteus scores.

The correlations were obtained from the formula:

$$R = \frac{xy - Cx.Cy}{\sigma x . \sigma y},$$

where X and Y are the two variables and R is the correlation coefficient between them,

σx = the standard deviation of the X 's around their true mean,

σy = the standard deviation of the Y 's around their true mean,

Cx = the difference between the arbitrary mean and the true mean of the X values,

Cy = the difference between the arbitrary mean and true mean of the Y values,

xy = the mean product deviations around the arbitrary means of X and Y .

In the nature of the case high correlations are not to be expected because the scores cannot be regarded as exact since they depend on so many variables. The correlation coefficients are in fact low, but yet are of some significance.

The group under discussion which is artificially restricted by choice in the Merrill-Palmer series shows quite considerable deviations in the Terman series. On the other hand the combined Terman-Porteus series shows rather less deviation, indicating that the deviations of the two tests tend to cancel each other out. The Terman test is of course much less definitely a performance test than is the Porteus test, and the above observation tends to show that the combination of the two is a useful record, since power of pure spatial manipulation (Porteus test) can be placed against the power of verbal manipulation (Terman tests) and the score evened out. It was held in a former paper (1) that spatial manipulation is as important as verbal manipulation. The nature of spatial manipulation as the basis of all language activity was discussed, and it was argued that the power of spatial manipulation is actually more fundamental than verbal manipulation.

The correlation coefficients were as follows:

Merrill-Palmer scores to combined Terman-Porteus scores	0.318
Merrill-Palmer scores to Terman scores	0.301
Merrill-Palmer scores to Porteus scores	0.093
Terman scores to Porteus scores	0.180

N.B. Porteus correlations with Terman tests for boys and girls of various ages range from 0.24 to 0.75, the higher correlation tending to occur in the higher age groups (2).

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From this it appears that although the correlations are by no means high, the correlation of Merrill-Palmer with combined Terman-Porteus scores or with Terman scores compares very favourably with the correlation of Terman scores with Porteus scores at this age, 66-78 months, and is of sufficient significance to suggest that the Merrill-Palmer scale and the Terman scale, or better still the combined Terman-Porteus scale, may be regarded as a continuous series for mental defectives, and that a comparable Intelligence Quotient may be estimated for all grades of defectives using the Merrill-Palmer tests for the low grades and the Terman tests for the higher-grade cases.

IV. THE PERFORMANCE OF MENTAL DEFECTIVES IN THE SEPARATE GROUPS OF THE MERRILL-PALMER TESTS.

In order to discover how the defectives performed in the four groups of the tests, the following procedure was adopted. It should be remembered that there are 93 individual tests in the Merrill-Palmer scale, and these are divided as follows:

- 18 in the first group (direct commands),
- 12 in the second (language),
- 33 in the third (motor co-ordination), and
- 30 in the fourth (puzzles),

so that a subject who scored a maximum would score 18, 12, 33 and 30 in the four groups respectively, and these may be described by the fractions, $\frac{18}{93}$, $\frac{12}{93}$, $\frac{33}{93}$ and $\frac{30}{93}$. Similarly if the total score in a given case is 51, we may show the group scores in the same way, thus, for example, $\frac{18}{93}$, $\frac{7}{93}$, $\frac{23}{93}$, $\frac{3}{93}$. This means that this subject scored 18 out of 18 in the first group, 7 out of 12 in the second, 23 out of 33 in the third, and 3 out of 30 in the fourth, and his total score is 51 out of 93. All the cases were scored in this way, and then reduced to a common basis by the following formula:

Possible total score is 93.

Possible group scores are 18, 12, 33, 30.

If total individual score is x , and individual group scores are a , b , c , d , then the coefficient of 1 is:

$$\frac{a \times 93}{18x}, \quad \frac{b \times 93}{12x}, \quad \frac{c \times 93}{33x}, \quad \frac{d \times 93}{30x}.$$

This in the example given above would be:

$$x = 51, \quad a = 18, \quad b = 7, \quad c = 23, \quad d = 3,$$

$$\frac{18 \times 93}{18 \times 51} = 1.8, \quad \frac{7 \times 93}{12 \times 51} = 1.06, \quad \frac{23 \times 93}{33 \times 51} = 1.27, \quad \frac{3 \times 93}{30 \times 51} = 0.18.$$

These figures, 1.8, 1.06, 1.27 and 0.18; give a measure of the comparative ability of this individual in the four groups of the test. It can be seen that this particular defective girl had no difficulty in performing direct commands (group 1), much more difficulty in language tests (group 2), less difficulty again in motor co-ordination tests (group 3), but very great difficulty again in the puzzle tests (group 4). It should be noted that these figures express comparisons with each other and have no absolute value, *i.e.* the score 1.8 in the first group does not imply a supernormal ability, but that her ability in group 1 is to her ability in group 4 as 1.8 is to 0.18.

If we take one other example at random whose total score is 60, we find that the coefficients are 1.50, 1.50, 1.19, and 0.26. Again there was great difficulty in group 4, equal facility in direct command tests and language tests, but less facility in motor co-ordination tests. Although the whole 254 were worked out in this way, to give the tabulated figures for them all would obviously be tedious and confusing, and the best way to display the relative abilities in the four groups seemed to be to find the true mean in each group for the whole 254 cases. The figures of true mean and probable error for these groups are as follows:

Group 1 (direct commands): true mean 1.292, probable error 0.033.

Group 2 (language): true mean 0.93, probable error 0.011.

Group 3 (motor co-ordination): true mean 1.099, probable error 0.013.

Group 4 (puzzles): true mean 0.740, probable error 0.024.

Since the probable errors are in all cases small, it may be taken that the figures are sufficiently reliable. Had the figures of the true mean in all cases been 1.0 it would have meant that this group of 254 defectives showed equal facilities in the four groups of tests. As it has turned out, they show considerably greater facility in obeying direct commands (group 1) involving simpler spatial manipulation than in any of the other tests, without the intervention of any complex symbolic formulation and expression (*i.e.* language) (cf. Head (3)). Next in order of facility come the tests of motor co-ordination (group 3) which do not involve any complex symbolism or other intellectual intervention, but a certain degree of spatial manipulation and discrimination. Then slightly below the expectation (group 2) are the language tests which definitely require what Head calls symbolic formulation and expression. Finally, the puzzle group (group 4) is on the whole badly done, involving as it does much more complex spatial discrimination and manipulation, and some symbolic formulation and expression. If it is correct to assume that

spatial discrimination and manipulation is the basis of language and of the higher intellectual faculties generally, then the groups of tests in order 1, 3, 2, and 4 represent increasing complexity of this, and the abilities shown in them by these low-grade defectives show the expected variability. This agrees with our findings in other experiments on this type of case in relation to spatial manipulation and discrimination (1).

While the defectives, therefore, do perform the tests in the same sort of way as do the normals, language falls from the second to the third place in difficulty. In certain of the other tests referred to (1), it was found that once language was mastered, *i.e.* in the high-grade cases, simple verbal formulations were easier than spatial formulations, and it is to be presumed from the present results that on the whole the normal pre-school child attains a better command of verbal formulations than does the defective of corresponding mental age.

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(Manuscript received 10 April, 1933.)

FACTORIAL ANALYSIS AND ITS RELATIONSHIP TO PSYCHOLOGICAL METHOD.

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CURRENT literature indicates a widespread interest in factorial analysis applied to human abilities. Among the writers who are contributing most to this field, an encouraging degree of harmony and co-operative advance is manifest; and spirited discussion adds zest to the searching enquiries being prosecuted along promising and vital lines. Occasionally, however, a note of partisanship, of hostility to one or other formulation, creeps in; and, as is usual in such cases, misunderstanding is likely to enter, since non-essentials are transformed into basic points of difference, and fundamental issues become clouded.

A major source of confusion lies in the failure to distinguish between *scientific method*, and the *techniques* employed by scientific procedure. This constitutes a serious hindrance to progress in any field; and its influence is felt particularly in psychology. Accordingly the time seems opportune for its careful consideration, with especial reference to the study of individual differences.

The term 'scientific method' (as used in contra-distinction to 'technique') always refers to the universal and basic aspects of enquiry. More particularly, it denotes the working approach to a field of search whereby that field is marked off from others. By thus converting the general definition of a branch of science into one that is sufficiently precise for practical purposes, it indicates the problems it will attempt to illuminate, at the same time excluding all others from its purview. The techniques it will employ depend upon this preliminary decision as to method, and are therefore secondary to it from a systematic point of view. They are devised or chosen as means to reliable observation of phenomena; but the phenomena themselves are patterned according to the method. Consequently it follows that techniques have little or no definite meaning apart from their methodological origins; and hence these latter must be thoroughly appreciated before the techniques can be evaluated or criticized.

Once this fundamental distinction is recognized, a number of corollaries become clear. In regard to our present problem, the most important of these are:

(a) Psychological method must be determined prior to the selection of techniques adequate to its purpose.

(b) The method must be of such a nature that it is pertinent to the problems to be investigated. It can be shown that these problems always originate in a practical (social or personal) interest, and hence reflect the general social philosophy of the time, and the relation of the individual scientists thereto.

(c) The method must be adopted throughout with consistency. This does not prevent an individual scientist from investigating along the lines of several quite distinct approaches; nor does it prevent him, in his applications of scientific findings, from drawing on different branches of science or widely divergent psychologies for results that may be pertinent to various aspects of his practical interests. But it does mean that scientific research at any one time must be systematically pure, in that it adheres to one method. It is in this latter sense that the term 'consistency' is here intended. Only thus can a true 'system' of investigation result.

(d) The method should offer a system of research that is comprehensive. This means two things, and therefore offers a twofold possibility of critical evaluation. First, we may ask whether any given scientific approach promises a comprehensive survey of the problems and interests from which it originated. Secondly, does the method make possible a survey of the field that is logically complete, as against a haphazard series of discoveries that seem to have no necessary interconnection? If the latter is true, the direction of the enquiry is obviously at the mercy of chance to a large extent, and the results will consequently be lacking in form.

Let us now consider these principles as they apply to psychological approaches involving the techniques of factorial analysis. Since these techniques were first devised and employed by Spearman, we may take his contribution as an example.

The salient feature of Spearman's work is that his primary interest is the psychological study of behaviour. Now 'behaviour' becomes specifically defined when methodology puts its stamp upon it. For Facultism, it is one thing; for Associationism, another; for Behaviourism, yet another; and so on. Each approach attacks certain problems, concerns itself with certain phenomena; and these, growing out of a practical

or personal interest, themselves indicate the specific nature of the field that finds expression in the methodological postulates of the 'system.' So that Spearman's view of, or approach to, the study of behaviour will reflect his broad interests, the problems he is desirous of solving as a scientist; and it is only by a thorough understanding of the resulting method that we can appreciate the significance of any techniques that he has employed¹.

What, then, is Spearman's method²? Originating in an interest in education broadly conceived, it demands a view of 'behaviour' that is adequate to the understanding of human development. At the very outset, therefore, it purposely discards mechanism. Any psychology founded upon mechanism or structuralism will be too narrow; for the 'behaviour' of human beings, viewed in human terms, seems to extend beyond mechanism, movement, interaction between static atoms of experience or of matter. Organization, purpose, doing something, adjustment, and the like, must be given recognition. We must approach our study so as to give place to the essentially practical co-ordinations manifested in behaviour, including, at the same time, the main advantages of more limited views. It is in this comprehensiveness of aim that Spearman's system embraces many narrower approaches; and though this is not without its dangers, since it may seem to lead to the systematic impurity known as eclecticism, it is an attempt to give the greatest degree of synthesis to psychological diversity. Thus interpreted, its author is perfectly sincere and scientific in dubbing it "A School to End Schools" (5)³.

But such a view is essentially dynamic—functional in the true sense of that much abused word⁴. The question raised is essentially that of

¹ The spurious application of statistical tools, against which the more insightful statisticians warn us repeatedly (1), reflects a failure to appreciate this point. Equally dangerous, and growing out of similar misunderstanding, is the erroneous supposition that the techniques are abstract, logical beginnings; and that from the data they give, we proceed to psychologize according to our several prejudices. This seems to be one of the fundamental weaknesses displayed by Tryon (14, Introduction); and by a few statisticians, who, from their mathematical logic, argue concerning psychological matters without due consideration for the methodological implications of their arguments.

² The interpretation of Spearman's method here given is an expression of the present writer's appreciation of that method, rather than a restatement of anything that Spearman himself has written.

³ Because of current misunderstanding or looseness of expression, it should be pointed out that the term 'school' does not strictly apply to the two-factor aspect of Spearman's work, but rather to his psychological method.

⁴ The writer has elsewhere suggested the term 'process-' as a less equivocal substitute for 'functional-' psychology (2).

understanding the nature of 'behaviour,' *how* the individual achieves his goal, does this thing; and the terms ultimately used in describing development will be those found necessary to the description of behaviour. In determining what terms will reflect most clearly the dynamic nature of behaviour, some form of analysis must be introduced; and Spearman is convinced that the analysis is possible in *psychological* terms, provided that adequate techniques can be discovered. He demands that the dynamic nature of behaviour be preserved at all stages of the analysis—an ideal that earlier functionalisms have recognized but never achieved. So that his experimental techniques will be all the more significant to psychology if they assist in satisfying this demand.

Before proceeding to discuss the techniques themselves, we must examine the method in greater detail. In doing this, it will be helpful to remember that any method must be pertinent to the problems originating in a practical interest. The details of the method will therefore depend upon or reflect the characteristics of that interest. Thus the demands made by formal education in the days of the Schoolmen quite naturally led to a facultistic view of development. As realism in education came to the fore, in post-renaissance days, empiricism provided the psychological justification for curricula units, and the necessary direction of teacher-training. It was much more suited than was faculty psychology to the practical education of the many, rather than to the discipline of the few. When the rise of nationalism and industrialism led to mass education, the pragmatic simplicity of atomistic functionalism was equally obvious as a psychological method of search, devised to meet the needs of existing conditions. If we can detect, therefore, those practical problems behind Spearman's method, we shall be in a position to appreciate more clearly the true nature of his work.

As has been stated above, the practical interest that leads to Spearman's approach in psychology is an educational one. He is first of all concerned, therefore, with an understanding of what education is trying to do. Now the point of view of the educationist may be conveniently expressed by considering the special task for which schools exist. And here we may conveniently confine ourselves to professional education of the traditional type, rather than to any of the revisions offered by radicals and individualists. The school as a social institution exists in order that the abstract tools and values arising from race experience may be transmitted to the present generation by short-cut methods. This procedure is necessary in the interests of social adjustment. It is the only way of ensuring the possibility of further progress in human creativity.

All learning is achievement; but the present generation must acquire the significant abstractions of our ancestors in order that their creations may go beyond the already known. In our inadequate attempts to cope with mass education, we may have lost sight of this purpose to some extent, and mere details may have become final goals. But the true aim of the school is to direct creativity, to present the abstractions of earlier thinkers in a setting where they may be immediately grasped, so that man's enjoyment of creation may take place at still higher levels.

Thus defined, there may appear to be an undue stress upon intellectual attainments, to the detriment of personality development, emotional stability, mental well-being. As a working definition of field in education, however, it serves a very useful purpose, and assists in directing the educator's orientation to problems of development. Intellectual creativity cannot go on to its fullest extent unless other aspects of human growth are adequately maintained; so that indirectly he is interested in them also. Still, he will ask, how do these less easily defined phases become trained? And the answer seems to be, that they develop in proportion as intellectual insight progresses. We do not train the emotions directly, but only ensure their adequate functioning in an intelligent scheme of behaviour. Any lack of personal balance represents inefficiency of intellect, so that educational methods should do no harm to personality growth; and the educator must be able to diagnose personality defects, for these hamper his progress along the lines of his special task. Consequently, disparity between achievement and basic intellectuality is of special concern to him; and the manner of diagnosing and interpreting such disparity is all-important in insightful education. But the central feature of his task is the fostering of intellectual progress.

In an educational outlook of this kind, mechanistic psychologies are somewhat out of place. There appears to be a need for portraying the dynamics of man's creativity, the basic processes of thought, and the laws governing their working and development. Such—according to the present writer's understanding of Spearman's work—is the all-important interest behind the doctrine of Noëgenesis (10).

In what way does this interest differ from those of other functional psychologists? A highly significant point of difference lies in the search for factors which illuminate the *general* aspects of development. Since this characteristic of Spearman's method is unique, we must discuss it at some length, and endeavour to contrast it with multiple-factor systems wherein the dynamical unity of behaviour is destroyed.

Without exception, all previous forms of functionalism have been

based upon arbitrary analyses. By this we mean that the criteria governing and directing the subdivision of function have been taken from disciplines outside the realm of psychology. Facultism, for example, employed distinctions of transcendental philosophy (such as that involved in differentiating 'perception' from 'thought'); Behaviourism, those of atomistic neurology; Hormic psychology, those of biological needs; the psychology of special abilities (including such very detailed varieties of this as Thorndike's earlier formulation), those of education; and so on. Whenever this artificiality permeates the analysis of function, structural analogies are sought to justify the subdivision; and the dynamics of function are lost sight of thereby. Further, the organism itself has become a bundle of independent units; so that the problem of integration is very real, but insoluble, since it involves dynamics of another order—namely, cross-function, synthesis, co-ordination. Statistically, too, the question of 'functional unities' is raised; but since that arises more concretely after the requisite techniques are employed in a systematic setting, we may pass it by for the present. We should note, however, that all these problems grow out of functional analysis directed by arbitrary, non-psychological criteria. They must be very carefully considered by those who are 'biased' in favour of multiple-factors¹ as a basis for building up an understanding of behaviour.

As against the *a priori* analyses above illustrated, Spearman's method starts with unified, integrated, organismic behaviour, and endeavours to preserve that unity throughout. Any analysis that takes place must be directed by the techniques of experiment, and must lead to more insightful understanding of behaviour in *psychological* terms. Despite the fact, therefore, that Spearman arrives at a multiple-factor view of functioning, his formulation is the antithesis of what is usually intended by the term 'multiple-factor theory'; for, in his system, the sole basic functional factor is *g*, from which we derive our fundamental dynamic processes.

The contention made in the last paragraph is extremely significant in that it recognizes the unique nature of Spearman's approach; and we shall return to its substantiation later. At this point we must note some other defects of previous functionalisms; for, by seeing how they are avoided by the London school, we shall better be able to appreciate the scope of the method under consideration.

In discarding the 'mind' of the facultists, psychology became extremely analytical. Structuralism viewed development in terms of

¹ To the writer, Tryon's articles (13, 14) illustrate this point very clearly.

increasingly refined units, assimilated in the experience of the individual; and the principal condition necessary to effective assimilation was contiguity, with its subordinates, frequency, recency, intensity and the like. When functionalism returned, the analytical damage was done. Mind, replaced by mental states, now became a manifold of functions. These were innate, as were the faculties; like them they were strengthened by exercise. But their simplicity as depicted by the stimulus-response terminology seemed to be sufficient without any teleological implication. A mechanistic facultism, infinitely pluralistic, had arrived. With those refusing such a reduction of human nature to mechanical motion, purposivism lived on in the instinct-faculties; and with mechanists, the same pseudo-mentalism persisted in the subsidiary terms of the system. Thus Thorndike insisted upon 'conduction units,' upon 'right' responses as being significant in determining satisfaction; and Watson spoke glibly of adjustment, organized behaviour, and the like. Inconsistencies of this kind inevitably occur when behaviour acts (as dynamic wholes) are described in terms of part-functions.

One further weakness in multiple-factor functionalisms must be noted. They invariably involve 'exercise' or 'growth' as their basic principles of development. Structuralism was more sound here; for growth was not basic, but merely indicative of development conceived as a summation of experiences, and 'frequency' was not interpreted as 'repetition,' save of the conditions of experience. With functionalism, however, development has hitherto been explained by 'exercise' of function (where practice seems to be concomitant with progressive change in function), or by 'maturation' (if practice seems to have no effect, and yet progressive change occurs). But these terms are meaningless for science. Whenever they occur in scientific discussion, they are descriptive summaries of events really viewed and recorded in terms of the fundamental concepts of the discipline in question. Biological sciences do not tolerate them in any other sense. Yet functional psychologies have accepted them as 'principles' of development—unquestioned analogies, grossly misapplied (2)—without any attempt to describe the effects of exercise, or of growth, in psychological terms. Further analogies (bonds, traces, etc.)—all implying a substantive mind as a storehouse, a *tabula rasa* or nervous system—have been dragged in; and 'practice' and 'exercise' have tended to become synonymous. It is true that in recent years some writers, notably those of the Berlin Gestalt school, have seen the fallacy of this latter presumed identity. But even then, physical structure has supplied the main clue for explaining what activity does by way of

changing the organism. Especially in American versions of Gestalt, organization has become linked with neural patterning. In fact, it is only recently that we have sought evidence to prove the obvious—namely, that exercise has no effect *as* exercise, but only by permitting change of process (3). Such, again, is the penalty we pay for going outside our field for basic distinctions within that field. Multiple-factor theories must certainly pause, before giving rein to their bias towards indiscriminate analysis.

We are now in a position to see how Spearman avoids the defects above discussed. Starting with behaviour—unanalysed, purposive, dynamic—he evaluates different behaviour acts in such a way as to show *relationships* between them. This involves the techniques which he has devised for demonstrating partial identities (7). Finding an unquestionable relationship symbolized by *g*, he proceeds to experiment with a view to interpreting that relationship functionally. It matters not, for his system, whether *g* may be viewed as a manifold (by other approaches) or as an entity. For his system, *g* is *g*, and not an indefinite average-of-capacities. The resulting principles of noëgenesis, as a process-interpretation of this relationship, are basic indications of creative functioning, truly scientific in their simplicity, and capable of experimental demonstration. Incidentally these principles not only assist our understanding of the real nature of human creativeness, but also give us the fundamental concepts necessary to the scientific use of the terms 'exercise' and 'growth' (2)¹.

¹ The concept 'growth' can hardly be dismissed in so cursory a manner, yet it is a difficult one to discuss without going very far afield. The writer's position in regard to it may be made more clear by the following brief note:

There is an interesting parallel between the evolution of philosophic concepts and the genesis of concepts in children, a parallel that is very pertinent here. Especially is this true with regard to the early Greeks. It is doubtful if the static connotation given by Aristotle to the 'primordial substance,' 'beginning,' as viewed by the earliest cosmologists, really represents the true meaning of the philosophies in question. Rather does it appear that early philosophic consciousness apprehended an essentially dynamic totality, purposefully organized and ordered within itself, and known to them not as substance, but as becoming. The fact that 'time' is the earliest concept would substantiate this. So with the child. Modern psychology now views his experience as apprehended directly in dynamic characters. To express dynamics in language, however, is almost impossible, at least without a great deal of circumlocution; for language is itself structured as crystallized concepts having a definitely static appearance. Thought thus proceeds to seize upon aspects of the directly apprehended dynamic, and uses these as the terms most convenient for expression. In the process, dynamics are temporarily lost, and have to be replaced by new terms.

Psychology reflects this difficulty very clearly. The growth process is understood in that it is indefinitely apprehended in experience. But to understand it further, we structuralize

In these principles, the term 'relations' plays a dominant rôle. Now relations may be classified in various ways. A logical classification may serve to guide certain forms of experimentation, since by logic we may ensure quite arbitrarily that all possible relations are taken care of by our system of classification. Such a system Spearman gives us (8); and he uses that system to corroborate his interpretation of g (9). For other purposes, a totally different form of classification may be necessary; but always the need for comprehensiveness must be kept in mind.

Let us carry this point further, and see how Spearman is able to discover other factors without destroying organismic unity, and without violating the scientific demand for comprehensiveness. We have noted that g —a qualitative relationship between behaviour acts—becomes functionally meaningful in the doctrine of noëgenesis. But every manifestation of function is an event, and therefore involves a specific aspect. This fact is symbolized by the s factors. If our interpretation of this system is right, these 'factors' are not to be regarded as specific functions, but rather as particular directions of function involving further possibilities of individual differences. They represent the degree of cognitive organization¹ already achieved relative to the manifestations of dynamics in question; and in tests of g this organization is largely confined to the fundamentals between which critical relations must be deduced. But even though the s 's are not functional factors themselves, they offer the possibility of indicating directional interrelationships in behaviour that lie at the root of achievements in particular fields of behaviour. The

it by discussing different 'stages.' These have no real right to the self-sufficiency they thus take on, since they are but the product of the 'before,' and the cause of the 'after.' In this cross-sectioning, Associationism analysed the 'stage' into atoms, and worried about the genesis of a cross-sectional pattern, the unity of phenomena, and the cumulative storage of atoms in memory. *Gestaltpsychologie* stresses the organization of phenomena as basic to experience, thus avoiding atomism; but in stressing also the unity of experience in time (longitudinally viewed, as we say) it relies to a large extent upon the concept of maturation. Our plea here is that while growth is undoubtedly a basic concept in philosophy, and is prior to the analyses of science which further depict growth, the method of science is such that some form of analysis is inevitable in making the term useful. Psychology has used it naively, and has not borne its philosophic priority in mind; and we here argue for a dynamic view of growth that avoids the static atomism of Associationism, and the unexamined maturation of Gestalt, yet which represents the process of growth by clearly apprehended processes. Spearman's doctrine of noëgenesis seems to have achieved this to a much more satisfying degree than has any other system.

¹ 'Organization' does not here imply neural, structural bases. It is intended as it would be depicted by the dynamic laws of noëgenesis. Conceptions of 'memory' in terms of traces, neural patternings, mental storehouse, and the like, are not necessary in the system here under consideration. (For the more careful elaboration of this point, see (2).)

group factors, in other words, reflect aspects of personal development that constitute significant conditions of function.

How can these aspects be surveyed comprehensively? They may be found in various behaviour settings—settings defined or classified according to non-psychological criteria. Education has suggested 'special ability' classes, which have not been very fruitful for directing research of this kind. That an 'arithmetical' group factor seems to exist under certain conditions, is only an interesting demonstration of the group-factor technique. Spearman's classes of relations are similarly inadequate for our purpose, since they are but logical conveniences accepted for reasons already stated. In his own work, they do not play a very significant rôle in the experimental survey of group relationships. A truly psychological scheme of classification appears to be demanded; and by that we imply a scheme in harmony with the postulates of the method that is adopted from the outset. The writer has elsewhere suggested certain of the major lines of such a scheme (2), and has indicated the relation thereto of Spearman's factors other than *g*. Throughout, it should be emphasized that the one basic functional factor is *g*, from which we derive our fundamental dynamic processes; the other 'factors' are of a different order, since they indicate organismic conditions of function.

Factorial analysis may therefore be conducted as a technique within a psychological method, in such a way that functional unity and integration may never be destroyed. Spearman's multiple factors can never be multiple functions; and those that indicate directional interrelationships are merely designed to depict the particular plan of integration manifested in the individual. Carried forward along the lines of a comprehensive psychological scheme, this latter aspect avoids all those limitations of atomism that we have noted above. Particularly does it make possible that degree of systematic consistency demanded by our original postulates (see p. 188)¹.

¹ The inconsistency of multiple-factor views, statistically fostered or otherwise, has been noted above, particularly in connection with problems of synthesis and integration (see p. 193). At this point we may add that so long as Thorndike adheres to his original approach to behaviour, the question of interrelationship between bond-efficiencies can never come within the purview of his psychology. The postulates of his system quite definitely exclude this question; for they are designed to direct our understanding towards the conditions under which efficiency may be increased, and do not in any way contribute to the systematic analysis of function. (This latter point is particularly obvious when we note that Thorndike's hypothesis of manifold *S-R* units was arrived at *a priori*, and as a convenient formula for systematizing his studies of efficient conditions of function.) If, therefore, he accepts the validity of *g*, or of any analogous relationship, as he seems now inclined to do (12), he must in this respect be thinking in terms of a totally different system from that which

In conclusion we would reiterate that since factorial analysis involves techniques employed by different psychological methods, the methods themselves must be understood before the findings of the techniques can be appreciated. Spearman's approach is not a functional atomism; that usually intended by advocates of multiple factors undoubtedly is, for it arises out of post-evolutionary analytical psychologies. This distinction is all-important in evaluating the critical discussions in this field. Yet it is frequently overlooked, with disastrous results. Many of the statistical techniques employed by factor-analysts are themselves dependent upon assumptions that place these techniques within a definite systematic approach¹; and many of the arbitrary settings devised to prove psychological hypotheses involve similar methodological postulates which vitiate the result, yet which pass unnoticed². The only basis of true criticism is a rigidly systematic one, such as has been here briefly outlined.

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resulted in his non-focal view of intelligence. He has ceased, in that admission, to adhere to the approach usually signified by the term 'multiple factors.' Similar inconsistencies emerge in some of his recent contributions (1), where he introduces concepts that belong more properly to a dynamic mental functionalism.

In contrast to all this, we are forced to pay tribute to the invariability of purpose manifested in Spearman's work, since he has rigidly adhered to his method as stated in 1904 (6), and to the consistently dynamic approach made available by his method.

¹ As an example, Tryon (14), p. 330) appears to disprove the two-factor interpretation of zero tetrads by using a formula which itself holds only when the two-factor implications of function obtain.

² Dice throwing offers a frequently encountered illustration. See, for instance, Tryon (14), p. 332 n.), who seems to disprove the obvious and logically necessary proposition, that "under the independent multiple-factor theory, a large number of factors would lead to all individuals obtaining the same total score." His 'proof' overlooks the fact that the dice-situation described by him can never give multiple factors involving independence throughout.

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(Manuscript received 4 April, 1933.)

AN INVESTIGATION OF FACTORS INFLUENCING THE VISIBILITY OF FLICKER UNDER CONDITIONS OF MONOCULAR STIMULATION¹.

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- I. *Introduction* (p. 199).
- II. *Description of apparatus and procedure* (pp. 199-201).
- III. *Experiments and results* (pp. 201-7).
 - (a) *A preliminary survey (statistical)* (pp. 201-2).
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I. INTRODUCTION.

WHEN the two retinas are stimulated independently, the one by a faint flickering light, and the other by a bright light of constant illumination, a phenomenon occurs which may for the purpose of the present study be defined as binocular perception of flicker with monocular stimulation. That is to say, when the flicker is above the limen of visibility for the stimulated eye it dominates the entire field of vision as though both retinas were stimulated by the flickering light. If the flicker is observed as a liminal experience, it will be seen to disappear and reappear in accordance with the law of retinal rivalry. It is this binocular phenomenon that is of special interest and raises a psychological problem. How will the perception of flicker in the present situation be affected by complicating the stimulating conditions? The object of the present experiments was to determine the effect of certain configurational factors upon the just perceptible binocular flicker under conditions of monocular stimulation.

II. DESCRIPTION OF APPARATUS AND PROCEDURE.

The requirements of the experiments were fulfilled by means of the following apparatus and procedure. A box 42 in. square and 24 in. deep was divided by a partition into right and left compartments (Fig. 1). In

¹ This study was begun at Cambridge University at the suggestion of Prof. F. C. Bartlett and was completed at the University of Montana.

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the right compartment a Mazda lamp of low wattage (25 watts in one experiment and 15 watts in another) was inclosed in a light-tight housing except for a 2-in. hole in the front partition into which was inserted the shutter of a photographic camera, with lenses removed. A disc with a $1\frac{1}{2}$ in. opening was rotated eight times per second in front of the shutter by means of a constant speed motor. Thus eight flickers per second were produced in the right or flicker compartment in such a manner that the duration of light was 0.01 sec., while the dark interval was 0.11 sec.¹ The left compartment was illuminated by a 200-watt lamp of constant

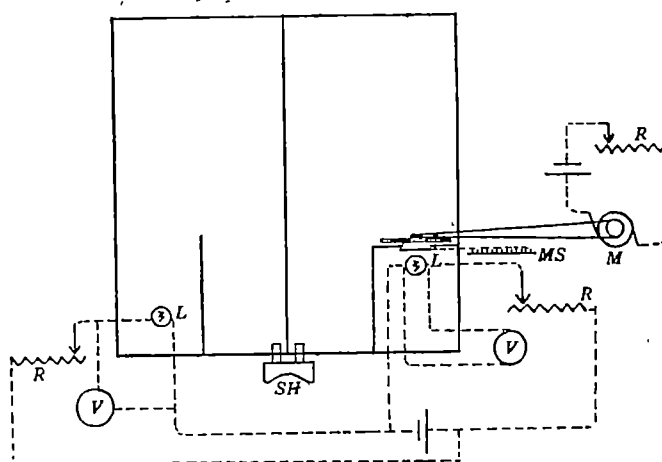


Fig. 1. Light box. *L, L*, Mazda lamps; *R, R, R*, rheostats; *M*, a constant speed motor; *V, V*, voltmeters; *MS*, millimeter scale; *SH*, stereopticon shade and eye tubes.

illumination. This may be referred to as the steady light compartment. Each lamp was on a rheostat control and was maintained at 112 volts. A 1-in. tube or eyepiece was inserted in the front of each compartment at approximately the interocular distance. The limen of flicker was obtained by alternately increasing and diminishing the aperture in the shutter. This was accomplished by means of a wire attached to the shutter and projecting through the side of the light box, the distance being measured on a millimetre scale, which was in turn calibrated in foot-candles of illumination on the test-patch on the back wall of the light box.

The limen of sensitivity of flicker in these experiments was dependent not on the absolute intensity of the flickering light, but on the ratio of

¹ Since the critical frequency for the complete fusion of intermittent light stimulations and the consequent disappearance of flicker is influenced by the intensity of light (i.e. the lower the intensity, the lower the critical frequency) it was necessary to select a frequency low enough to allow for very low intensities (0.01 foot-candle on the test-patch).

this intensity to the intensity of the constant light, that is, in these experiments the ratio of x f.c. to 23.42 f.c. However, since the denominator of the ratio remains constant throughout the experiments, this factor may be ignored in the computations, and the results may be expressed in terms of the variable light. Table I below gives the illumination in f.c. of variable, *i.e.* flicker lamps used in these experiments.

Table I.

Constant illumination, 200-watt lamp, 23.42 f.c.

mm.	Variable illumination	
	25-watt lamp f.c.	15-watt lamp f.c.
0 (open)	1.160	0.578
1	1.114	0.558
2	1.069	0.536
3	1.027	0.509
4	0.984	0.494
5	0.928	0.472
6	0.871	0.448
7	0.827	0.431
8	0.774	0.414
9	0.731	0.395
10	0.683	0.376
11	0.616	0.357
12	0.581	0.339
13	0.542	0.294
14	0.503	0.267
15	0.465	0.246
16	0.387	0.212
17	0.356	0.188
18	0.302	0.161
19	0.256	0.133
20	0.223	0.108
21	0.172	0.082
22	0.124	0.055
23	0.080	0.035
24	0.040	0.018
25 (nearly closed)	0.016	0.009

III. EXPERIMENTS AND RESULTS.

(a) *A preliminary survey (statistical).*

Before beginning the experiment to determine the effect of various configurational factors upon the limen of flicker perception it was thought desirable to make a preliminary test on unpractised observers for the purpose of orientating the problem and method and of revealing certain statistical results.

A single test included 100 judgments by the method of limits, 50 judgments with fixation on the blank background and 50 with fixation

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on a 5-in. black disc¹. By a judgment is meant a just perceptible flicker or a just not perceptible flicker. The limen of flicker under the conditions of the experiment is measured in terms of the f.c. of the variable light (in the present experiment, a 25-watt tungsten lamp). This lamp gave a maximum of 1.160 f.c. with the shutter open and a minimum of 0.016 f.c. with the shutter nearly closed.

The observers were given the following instructions: "Look through the eyepieces with both eyes open and observe that the light is flickering. Say 'Off' the moment the flicker disappears and 'On' the moment it reappears." In this manner 25 observers, undergraduate students in psychology, took part in the preliminary test. A brief adaptation period of approximately 10 trials preceded each individual test. From later results it will appear that a much longer practice period is necessary to eliminate the effect of practice.

From the results of the preliminary experiment, it appears evident that the perception of flicker is enhanced by the fixation of an object in the field of illumination. In all but one of the 25 cases the limen was decreased when the black disc was fixated as compared with the limen when the flicker was observed against the blank background. The average limen when the eyes were fixated upon the uniform or blank background (hereafter designated as *B*) was 0.299. When fixating a black disc (*BD*) the average was 0.200. The average decrement was found to be 0.099, or approximately one-third. The one exception showed an increase of 0.023. The average s.d. for the 25 individual tests for *B* was 0.073 and for *BD* 0.050. The coefficient of variability is therefore seen to be 0.292 and 0.200 respectively.

A comparison of the reliability of the judgments in *B* and *BD* gives further support to the conclusion that the fixation of an object in the field of illumination tends to enhance the perception of flicker, *i.e.* lower the limen. In all but four cases the standard deviations from the individual means are less for *BD* than for *B*, the average decrement being 0.022, or about 30 per cent. The standard deviation of difference between the means of *B* and *BD* for our 25 cases was found to be 0.030, which yields a critical ratio of 4.48. Thus from every point of view the statistical results give conclusive evidence that some factor other than the flicker stimulus, but in combination with the flicker, is operating, which influences the limen of flicker perception.

¹ The black disc was cut vertically into right and left halves and each half was fastened at the centre of the back wall of the light box and at the level of the eyes.

(b) *Experiments designed to determine the influence of various configurational factors on the perception of flicker.*

The first experiment in this series was designed to determine to what extent the perception of flicker is affected by the size of a stimulating object. A comparison was made between the 5-in. black disc of the previous experiment and a 9-in. black disc. Three observers, advanced students in experimental psychology, were given 200 trials with fixation on each of these two objects, by the methods employed in the previous experiment. The total number of trials was distributed over four days of 50 trials for each observer on each of the two objects. Thus each observer made actually 400 independent judgments. It was discovered in the preliminary experiment that a 25-watt variable lamp was too strong for the actual limen to be obtained with some observers. It was decided, therefore, to substitute a 15-watt lamp in the present series. This lamp gave foot-candles ranging from 0.578 max. to 0.009 min.

A comparison of the limens for the 5-in. black disc and the 9-in. black disc gives an average of 0.078 for BD_5 and 0.072 for BD_9 . Thus a gain of 0.006 is made in favour of the larger disc which, although small, is consistent for the three observers and for each trial (Table II).

Table II. *Comparison of limens due to fixating small and large black discs.*

	5 in. BD	9 in. BD
C.	0.056	0.050
K.	0.098	0.088
N.	0.081	0.078
Av.	0.078	0.072

Next was a series of experiments designed to test the possible influence of such factors as stimulus-pattern, plasticity, and direction of attention upon the perception of flicker.

Three observers were given 200 trials with each of four objects in the field and also an equal number of trials with no object in the field¹. These were distributed over four days as follows: Each day's test was an exact duplicate of the preliminary experiment, that is, a test included 100 judgments by the method of limits, 50 judgments with fixation on B and 50 on one of the four objects. The test was repeated for each of the three observers, on each of the four days for each of the four objects. Thus

¹ The following objects were employed: a 5-in. black disc (BD); a 5-in. target (T); a 5-in. pattern of black and white bars (BP); and a figure in reversible perspective (RP) (see Fig. 2).

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each observer required 16 days to complete the experiment, making 1600 independent judgments.

The data from these experiments are presented together in Table IV. From an analysis of these results it appears that the fixation of the black

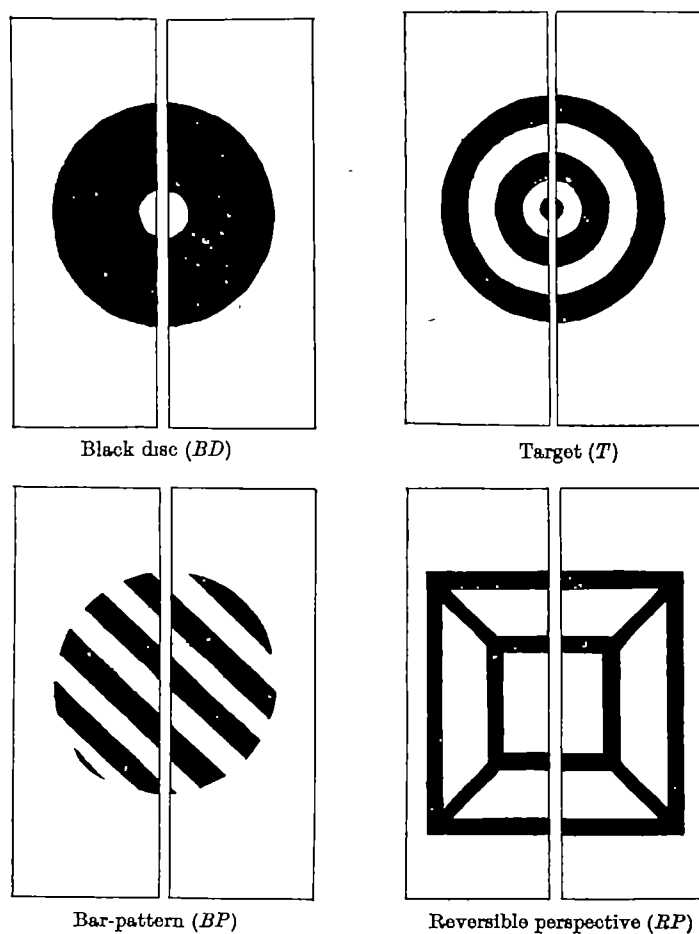


Fig. 2.

disc has the effect of lowering the limen 0.019; of the target, 0.018; of the bar pattern, 0.022; of the reversible perspective, 0.021 (Table III).

It will be remembered that each separate trial consists of two independent judgments, one when the flicker first appears and the other when it disappears. The foregoing results are based on the average of these two judgments. If the results of the two judgments are considered

separately it is found that the limen of disappearance is but slightly lower when the object is present than when the view is unobstructed by an object. The average limen of disappearance for *B* was 0.050, while the average limen for the four objects (*BD*, *T*, *BP*, and *RP*) was 0.038, or a

Table III.

	<i>B</i>	<i>BD</i>	<i>T</i>	<i>BP</i>	<i>RP</i>	<i>Diff.</i>
C.	0.085	0.070	—	—	—	0.015
K.	0.164	0.142	—	—	—	0.022
N.	0.089	0.075	—	—	—	0.014
Av.	0.113	0.096	—	—	—	0.017
C.	0.070	—	0.054	—	—	0.016
K.	0.164	—	0.138	—	—	0.026
N.	0.090	—	0.071	—	—	0.019
Av.	0.108	—	0.088	—	—	0.020
C.	0.102	—	—	0.086	—	0.016
K.	0.152	—	—	0.123	—	0.029
N.	0.092	—	—	0.070	—	0.022
Av.	0.115	—	—	0.093	—	0.022
C.	0.092	—	—	—	0.077	0.015
K.	0.158	—	—	—	0.137	0.021
N.	0.083	—	—	—	0.068	0.015
Av.	0.111	—	—	—	0.094	0.017

difference of 0.012. Table IV presents the data with regard to the point of disappearance of the flicker. The results show no appreciable advantage of any of the four objects over the blank background for the perception of the disappearance of flicker, although all the results are positive.

Table IV. *Threshold of disappearance.*

	<i>B</i>	<i>BD</i>	<i>T</i>	<i>BP</i>	<i>RP</i>	<i>Diff.</i>
C.	0.072	0.065	—	—	—	0.007
K.	0.109	0.086	—	—	—	0.023
N.	0.078	0.063	—	—	—	0.015
Av.	0.086	0.071	—	—	—	0.015
C.	0.028	—	0.015	—	—	0.013
K.	0.083	—	0.062	—	—	0.021
N.	0.024	—	0.016	—	—	0.008
Av.	0.045	—	0.031	—	—	0.014
C.	0.032	—	—	0.028	—	0.004
K.	0.069	—	—	0.052	—	0.017
N.	0.029	—	—	0.013	—	0.016
Av.	0.043	—	—	0.031	—	0.012
C.	0.014	—	—	—	0.010	0.004
K.	0.043	—	—	—	0.028	0.015
N.	0.025	—	—	—	0.019	0.006
Av.	0.027	—	—	—	0.019	0.008
Total Av.	0.050	—	—	0.038	—	0.012

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Table V presents similar data with reference to the just noticeable appearance of the flicker for the five situations included in the experiments. The decrement in liminal sensitivity to flicker for the four objects as compared with that for the blank background shows the same sort of variation among the objects in the average of appearance and disappearance. The average decrements are for *BD*, *T*, *BP* and *RP* respectively, 0.022, 0.028, 0.033, and 0.023. The average limen of appearance for *B* was 0.185 and for the four objects it was 0.156, thus yielding a difference of 0.029.

Table V. *Threshold of appearance.*

	<i>B</i>	<i>BD</i>	<i>T</i>	<i>BP</i>	<i>RP</i>	<i>Diff.</i>
C.	0.142	0.123	—	—	—	0.019
K.	0.238	0.199	—	—	—	0.039
N.	0.153	0.145	—	—	—	0.008
Av.	0.178	0.156	—	—	—	0.022
C.	0.158	—	0.138	—	—	0.020
K.	0.246	—	0.211	—	—	0.035
N.	0.168	—	0.140	—	—	0.028
Av.	0.191	—	0.163	—	—	0.028
C.	0.184	—	—	0.153	—	0.031
K.	0.240	—	—	0.196	—	0.044
N.	0.164	—	—	0.141	—	0.023
Av.	0.196	—	—	0.163	—	0.033
C.	0.149	—	—	—	0.131	0.019
K.	0.228	—	—	—	0.200	0.028
N.	0.151	—	—	—	0.128	0.023
Av.	0.176	—	—	—	0.153	0.023
Total Av.	0.185	—	—	—	0.156	0.029

It was thought that the contrast effect of the black figure upon a white background might influence the threshold of flicker. To test the probability of such an influence, the threshold for the black disc on white background was compared with that for a similar black disc on grey background. The difference, although slight, was positive for the white background (Table VI). Red and blue backgrounds were contrasted but the difference in brightness of these colours made the results uncertain.

Table VI. *Black disc on grey background compared with same black disc on white background.*

	Grey	White	
C.	0.156	0.138	0.018
K.	0.093	0.087	0.006
N.	0.085	0.077	0.008
Av.	0.111	0.101	0.011

Finally, a test was made to determine the possible effect of stereoscopic vision on the threshold of flicker. A copy of the stereoscopic slide, No. 14 Titchener's series, was drawn in black India ink on a transparent glass slide which was inserted in a slot in the partition between the two compartments of the light box. The hood and lenses of the stereoscope were fastened to the front of the box so that each lens covered the opening of one of the eyepieces. The two stereograms were thus viewed in stereoscopic vision. The results showed a very slight, almost negligible, but positive effect of stereoscopic vision on the just noticeable appearance of flicker. The average limen of appearance for *B* was 0.188 and for the four objects it was 0.160, thus yielding a difference of 0.028.

These results afford evidence of the influence of stimulus-patterns upon the threshold of the appearance of flicker. This conclusion finds corroboration in the introspections reported by the three observers during the experiment. These reports agree that the appearance of the flicker is sudden, clear, and definite even when there is no object in the field. The disappearance is gradual, the flicker fading away imperceptibly into a faint shimmer of light, then disappearing altogether. When an object is present the suddenness and definiteness of the appearance of flicker is appreciably enhanced. The flicker appears to shoot out from the dimness against the white background of the figure.

The averages given in Table V show that the target and bar-pattern exert a greater influence on the threshold of appearance of flicker than the black disc and the reversible perspective. The explanation of this difference would seem to lie in the greater plasticity of the stimulus-pattern represented in these two objects as compared with the black disc and reversible perspective. These latter present a solid black figure on a stable white background. Both of the other objects are likely to facilitate incipient eye movements and muscle strain which may enhance the perception of flicker.

It was expected that the reversible perspective would produce the greatest effect on the threshold of flicker, since it would appear to be the most plastic. The failure to produce the expected result can be explained by the fact that the shift of attention which normally accompanies the reversible perspective seldom, if ever, coincided with the threshold of flicker.

IV. CONCLUSION.

From the foregoing results we are led to the conclusion that the reaction to flicker under the conditions imposed by the present experiment, *i.e.* monocular stimulation of flicker, is not merely a retinal response, but a complex reaction involving configurational factors. Under these conditions it may be assumed that some dynamic factor in the objective situation combined with the flicker series to intensify the stimulus-pattern or configuration and thus intensify the reaction or, in other words, lower the threshold. Such a factor may be found in the contrast between an object and its background, as in the black disc and the square figure in reversible perspective, or in the plasticity of the stimulus-pattern as in the target and bar-pattern. The fact that when the flicker appears it is perceived in binocular and not merely in monocular vision would appear to be explicable on the assumption that central as well as retinal factors are involved in the reaction. In view of the objective conditions under which retinal rivalry occurs, this phenomenon would appear to lend support to the conclusion that we have here a perceptual response involving the blending of several factors rather than a simple sense-organ reaction.

(*Manuscript received 15 June, 1933.*)

PROFESSOR GODFREY THOMSON'S NOTE.

BY WILLIAM BROWN (*Oxford and London*) AND
WILLIAM STEPHENSON (*London*).

IN a recent note (1), Prof. Thomson expresses objection to the use, by one of us, of the phrase "proof of the theory of two factors." It is true that care is needed in this matter. But Prof. Thomson pursues, in his note, certain lines of thought which betray serious misconceptions, or narrow preconceptions, about tests of the applicability of the two-factor theory. Consider, for instance, the following paragraph from his Note:

"On no theory at all, by the laws of probability, there will be a tendency towards zero tetrad-differences among correlation coefficients. This tendency is the stronger the more complex the background of causes concerned in the correlations; and the mind is surely complex. When in addition the tests are selected 'after much preliminary trial,' and later it is 'found necessary to reject one of the tests' (*i.e.* to reject 2907 tetrad-differences) and then further to reject 'one of the correlation coefficients' which remain (*i.e.* to reject 272 more of the tetrad-differences), it would indeed be surprising if the surviving data did not show a very strong tendency to zero tetrad-differences. Nor," he adds, "can I for the life of me see what is then proved."

Three distinct particulars are here mentioned by Thomson, each with surprising naïveté.

First, Thomson's argument about the "tendency to zero tetrad-differences" is altogether too simple. Already, Brown and Thomson (2) p. 196) have stated that when the tetrad-difference criterion is satisfied, then "the Sampling Theory reduces to the Theory of Two Factors." But, they added, "every effort should be made to reduce the probable error" (of the tetrads). The hope was expressed, also, that Spearman and his co-workers would enter into this matter. As is well known, all this has been done, with attention to reduction in sampling error (3), and increase in number of correlated variables (4). But, still not satisfied, Thomson stresses that "on no theory at all" correlations will *tend* to fit the tetrad criterion, the implication being, it would seem, that it is futile to try to prove anything whatever.

We must reply that all depends upon what is meant by "tends to fit." And on this matter, indeed, the very technical developments that Thomson derides in his Note offer facts of a truly interesting kind. In point of fact, the work we do in tests of the applicability of the theory of two factors amounts to a search for the conditions under which correlation data satisfy the tetrad criterion. We seek to determine whether some data *might*, whilst other data will *not* under any circumstances, fit the criterion; it is the very object of our work to determine what is and what is not a tendency, what is and what is not acceptable as an approximation to zero tetrad-differences. Thus, Dr Stephenson's work (3), and that in collaboration with Dr Wm. Brown (4), have shown something of the conditions under which the tetrad criterion is satisfied; whilst another paper by Stephenson (5) has shown that certain correlations did not, and could not, be got to fit the criterion—even although every known or suggested allowance was tried out, and to a fine limit at least not less than that for which the previously mentioned data *did* satisfy the criterion¹. All this is necessarily technical and complicated, but it cannot be shirked on any theory of factors whatever.

We might add that any number of sets of correlations could be supplied which do *not* satisfy the criterion, and cannot be made to do so on any subsidiary theory whatever, just as there are others which *do* satisfy it under the same conditions of sampling error. Further, the conditions of satisfaction, as well as those of non-satisfaction, of the tetrad criterion are of direct psychological interest²—a matter scarcely of opinion, but in any case one about which non-psychologists should keep an open mind.

But, since these obvious technical matters are apparently not appreciated by Thomson, we are asked to remember that the mind is a complex affair, and that this should support a theory of innumerable factors as mental determiners. We freely concur on the matter of the complexity of the mind. But it is beside the point. It is of no great interest or value to us to verify that the mind is a complex affair. Purely as practical psychologists we require to separate firstly the broadest psychological features, if there are any, and to control them one by one. The two-factor theory is a device that helps us towards this broad objective. We would use the theory in two ways, firstly as a scientific check upon *a priori* theorizing, and secondly as an instrument which

¹ Incidentally, this latter work has been misunderstood by R. C. Tryon (6), and consequently quoted with its attendant mistake by Wynn Jones (7).

² See, for example, Brown and Stephenson, (4) p. 360, for a psychological account of the one rejected test, and the one rejected correlation, referred to by Thomson in the Note.

suggests prior facts, upon which we might base a *posteriori* theory. Thus, to give an example of the former procedure, we might introspect and make theories about Reasoning, Conceptualizing, Judgment, and Abstraction, as abilities showing individual differences. A scientific check could be that of a test of the disparateness of the abilities in question, and the two-factor theory can supply the needful check. Obviously, a large part of Spearman's work in *The Abilities of Man* is scientifically governed in this way.

A third matter raised by Thomson in his Note concerns what has been called the 'purification' technique, whereby certain conditions are placed on the correlations, or certain correlations are omitted. This, we have already said, has been misunderstood by Tryon (8), and it is apparent that Thomson shows the same fundamental misunderstanding. The technique takes two lines. First, a search is made for errors other than sampling—surely an eminently sensible aim, since we cannot see why sampling should blind us to the possibility of other errors. Second, a search is made for possibilities of 'overlap'—a matter made explicit in the very theory of two factors itself, and not, as Thomson seems to think, a further theory or additional excuse. The first search is made quite independently of any *particular* correlations. The second also entails an objective procedure: where 'overlap' is suspected, the matter is put to a scientific test, if this has not already been done, or acceptance of the 'overlap' is made contingent upon further work. Any remarks, then, about 'omitted tests' or 'rejected tests,' are as unnecessary as they are unsound.

Finally, it would indeed seem that Thomson still holds to an old view, that *any* mental tests whatsoever, under any conditions whatever, will allow a decision to be made, once and for all time, about the applicability of this or that theory. Nothing could be more naïve, nothing more mistaken. We have already said sufficient, it is hoped, to show that factor studies are by no means as simple as this old view holds them to be: but the various points raised above, together with others about which misunderstandings of the two-factor theory arise, will be discussed in greater detail later by one of us (Stephenson).

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(Manuscript received 13 April, 1933.)

THIRD REPORT ON A LAMARCKIAN EXPERIMENT.

By J. B. RHINE AND WILLIAM McDUGALL¹.

- I. *Introductory* (pp. 213-218).
- II. *The continuation of the tank experiment to the 34th generation* (pp. 218-221).
- III. *The behaviour in the tank of control rats of various stocks* (pp. 221-224).
- IV. *The continuation of the experiment combining training in the tank with adverse selection* (pp. 224-231).
- V. *An experiment combining training in the tank with favourable selection* (pp. 231-232).
- VI. *Brief answer to some criticisms* (pp. 232-235).

I. INTRODUCTORY.

THE first and second reports on my Lamarckian experiment, published in this *Journal*², stated and discussed results obtained in the first period (from 1920 to June 1926) and in the second period (June 1926 to June 1929) respectively. This article reports the continuance of the experiment through the third period (June 1929 to June 1933).

In this third period I have been chiefly occupied with two parallel tasks: first, the continued training of successive generations of the original stock of tank rats; secondly, the similar training of successive generations of a control stock subjected to severe adverse selection in each generation. I report on these two pieces of work in sections II and IV of this article. In section III I report some subsidiary control experiments; and in section VI the bearing of the experimental work is briefly discussed in the light of various criticisms. I restate here concisely the nature and conditions of the training process and add some few remarks on changes of the conditions. The rats are trained by a standardized procedure to avoid a brightly lit electrified gangway, in

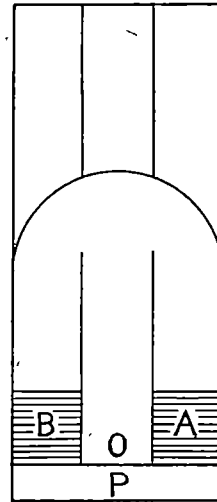


Fig. 1.

¹ I have written this report in the first person singular, but Dr Rhine has taken so large a part in the work that, with his permission, his name stands with mine as co-author. (W. McD.)

² (1927) xvii, 267 and (1930) xx, 201.

escaping from a tank of water, and to prefer always the only alternative route of escape, namely, a similar gangway dimly lit and not electrified. Fig. 1 represents the ground plan of the tank; *A* and *B* are the two sloping gangways.

Both are of ground-glass covered with wire-mesh and behind each is an electric glow-lamp of 60 candle-power. The tank is half-full of water and each gangway leads up to the platform *P* from a level just above the surface of the water. The wire-mesh on the gangway is insulated from the metal walls of the tank and from the water, but is connected with one pole of a secondary coil, the other pole of which is connected with the metal tank and thus with the water. If a rat, swimming in the water, places his fore paws on the wire-mesh of the gangway (while this is receiving the interrupted current from the coil) he bridges the electric gap, and the interrupted current, entering his fore paws, passes through his body to the water, and continues to do so, as he runs up the gangway, until his tail is clear of the water. The strength of the current which passes through the rat (and the intensity of the impression made on it, as judged from its behaviour) inevitably varies a little in ways which cannot be completely controlled (*e.g.* the kind of contact made with the wire by the rat's feet). After much consideration of this problem of standardizing the shock delivered to the rat, I am of the opinion that the best one can do is to adjust the secondary coil in such a way that the current is strong enough to tetanize the muscles of the rat's legs and hold him fast, in, as nearly as possible, 50 per cent. of all contacts made by any batch of rats. Further, when a rat is thus held by the current, I count three slowly before breaking the current and releasing the rat. In almost all cases the rat then runs up the gangway. When a rat touches the wire-mesh with paw or snout, it happens in rare instances that he turns back; but with these rare exceptions he (if not tetanized) rushes as quickly as possible up the gangway.

The two currents, that supplying the two lamps which illuminate the two gangways, and that to the wire-mesh on the gangways, are connected with one switch which, on being thrown over, diverts both currents from *A* to *B* (or from *B* to *A*). Each rat is placed in the water at *O* six times on each day of his training period, and escapes from the water six times on each day. After each immersion and escape, the switch is thrown over, transferring both currents from the one gangway to the other¹. After a sufficient number of immersions and escapes (a number varying widely) each rat (out of many hundreds subjected to this training process) learns to avoid the brightly lit and electrified gangway *B* and to escape regularly by the dimly lit gangway *D* (that from which he receives no shock). This training is continued day by day (with the occasional omission of training on one day) until he makes

¹ An occasional exception to this rule is made, in order to ascertain whether the rat has formed a habit of going alternately to right and left. In no case did any evidence of such habit formation appear.

twelve successive escapes without error¹, i.e. without touching (or approaching) the bright gangway. Each rat thus learns to discriminate between the bright and the dim gangway (in anthropomorphic terms, he learns to accept the bright light as a signal warning him of the shock) and to avoid the former. This discrimination is effected more or less suddenly, at a crucial point in the learning process, a point at which, it may be claimed, *insight* is suddenly achieved, that is to say an appreciation of the relation of the brightness to the shock. Until this discrimination is achieved the rats (with few exceptions) continue throughout the training process to make their escapes from the water equally often by the bright (*B*) and by the dim (*D*) gangway; the number of errors (or escapes by way of *B*) made by any rat in the total process of training is accepted as the measure of his facility in learning to effect the discrimination.

The rats fall into three pretty sharply definable classes in respect of their behaviour in the pre-discriminatory period of training: (1) rats (about 50 per cent. of all) which turn right or left and take route *A* or *B* irregularly and about equally often (they may be said to turn right or left 'at random'); (2) rats (a rather smaller number) which at an early stage acquire the habit of turning right and escaping by *A*, or left and escaping by *B*, and continue in this habit up to the critical stage of discrimination; (3) a few rats (probably fewer than 5 per cent.) which at some stage of the training process, after turning right or left 'at random,' acquire the habit of turning always to the bright side and escaping by *B*, and continue in this habit up to the critical stage of discrimination. It will be noticed that the rats of the first and second classes make errors to the number of approximately half the total number of immersions preceding discrimination; while the number of errors made by the rats of the small third class is a considerably larger proportion of the total number of immersions—in some cases almost 100 per cent. These diversities of behaviour do not invalidate (it seems to me) the acceptance of the number of errors made by any rat as the measure of his facility in effecting discrimination.

The interpretation of these diversities is subtly connected with the general theory of the learning process (cp. the chapter on the Learning Process in my *Energies of Men*). If we accept the common mechanistic theory, we shall assume that, up to the critical point of discrimination, the rat is learning not at all, is simply making reflex movements according to the sense-stimuli of each moment (class 1), or behaving according to a fixed habit (classes 2 and 3); and that then, for some unassignable reason, by the operation of some cause which no mechanist theory of learning can assign or suggest, but which must be regarded as some fortuitous combination of conditions (inner or outer or both) it falls into the new habit of turning always away from *B* and towards *D*. If this mechanist account were valid, the number of errors made

¹ The training process has been prolonged beyond this point in a few cases. Some of these rats make an occasional error, others (the majority) continue regularly to avoid the bright gangway on every occasion.

before discrimination would be to some extent invalidated as a measure of facility in learning, but not wholly invalidated. Even if the eventual discrimination is nothing more than a falling into a new habit through the happy chance of some favourable concurrence of conditions, it would remain true that the rats which learn after few errors must be regarded as in the main those most susceptible to the formation of the fortunate habit of turning towards *D* when conditions happen to favour such turning. But the mechanist interpretation is quite untenable in the light of impartial consideration of the behaviour of the rats. Though it may have a certain plausibility as applied to the rats of the first of the three classes, it clearly will not work for the rats of the second and third classes. These form a fixed habit (those of class 2, the habit of turning always right or always left; those of class 3, the habit of turning always towards *B*, whether *B* be right or left); and after a number of repetitions of this behaviour (varying from rat to rat from perhaps 10 repetitions to 200 or more) this habit (without any appreciable change of the external conditions) is abandoned, and the rat turns always away from *B* and escapes by *D*. Secondly, the rats show quite unmistakably by various details of behaviour that, as they approach the gangway *B*, they anticipate or expect the shock (as *e.g.* in rushing at and across *B* with maximum speed and energy after a few experiences of the shock). It seems safe to infer that, throughout the pre-discriminatory period of training, the visual perception of *B* evokes in the rat approaching *B* (after receiving the shock on *B* once or some few times) some anticipation of the shock and some aversive impulse which is not strong enough to inhibit or arrest the urgent impulse to escape from the water by any route that is visually recognized as such; further, that, with repetition of the shock on *B*, the anticipation of the shock before touching *B* grows more definite and the aversive impulse grows stronger, until (after a number of repetitions which varies from rat to rat) it is evoked in such strength as to counteract, inhibit or suspend the escape-impulse. When this stage is reached, when the aversive impulse evoked by visual perception of *B* has gradually (on successive repetitions of the shock-experience) grown so strong that it competes on equal terms with the escape-impulse, a new phase of behaviour sets in. The rat, instead of rushing at and over the gangway *B*, hesitates before rushing it. After repeating this behaviour once or several times, he passes to a further phase; namely, he hesitates on facing *B*, and then, instead of rushing *B*, turns back without touching it; in many cases, he will then turn repeatedly from side to side of the tank, looking alternately at *B* and *D*—evidently the seat of a well-balanced conflict of impulses. Having reached this stage of suspense of action he has accomplished the first stage of the process of learning and is ready to achieve the second stage, that of actual visual discrimination of bright-shocking-gangway from dim-safe-gangway. He may take *B* again once, or two or three times; but when this stage of suspended action has been gradually induced, a very few repetitions of the shock on *B* suffice to establish effective discrimination and invariable choice of *D* with total avoidance of *B*.

The total number of errors made by rats of successive generations trained in the tank is, then, the significant feature to which attention is invited. In order to effect a very considerable economy of my time and energy, I have trained the rats of each generation in batches,

generally of six animals (occasionally in rather smaller or larger batches), and I have recorded the number of errors made by the batch on each immersion. Thus I have no record (in the great majority of cases) of the number of errors made by each individual rat, and rely upon the figure expressing the average number of errors made by each rat of a given generation (obtained by dividing the total of errors made by all the rats of a given generation by the number of rats of that generation actually trained), together with the figures expressing the number of errors made by the best (or most quickly learning) rat and the worst (or most slowly learning) rat of each generation.

A complication of the training of the last few generations must here be explained. My original tank, constructed in 1920 (and also a duplicate constructed in 1927), contained certain wooden parts (the internal septa) which in the course of years immersed in water had become rotten. Therefore early in 1932 we constructed two new tanks to replace the old ones, following closely the patterns and dimensions of the old tanks, but making the septa of metal and introducing a few small modifications that made for convenience, such as increasing the height of the outer wall to prevent the occasional, but most time-wasting, escape of a rat from the platform *P*. No one who has not made similar experiments could imagine the great variety of ways in which the animals are apt to complicate the task of the experimenter by their idiosyncrasies of behaviour. I had hoped and assumed that these slight differences between the new and the old tanks would not significantly affect the behaviour of the rats. I was soon undeceived. It appeared at once that the new tanks are 'easier' than the old ones, that is to say, the rats solve their problem, or achieve discrimination, more readily, i.e. after fewer errors, in the new tanks than in the old. It was therefore necessary to standardize or calibrate the new tanks by means of exact comparison with the old, a task which occupied me throughout one academic year. This, I suggest in passing, is an instructive illustration of the difficulties of work of this sort. The comparatively simple instrument of my experiment, the tank, requires for its calibration a vast amount of work. The task cannot be accomplished by the simple procedure of training the same animals in the two tanks to be compared; but only by training a large number of animals in each tank and comparing the average facility of learning in the two tanks. Obviously, for this purpose it is desirable that the rats trained in the two tanks shall be as nearly alike as possible. With this aim in view, I chose my animals for this calibration work on the principle of splitting litters. That is to say, I took 21 litters of four different stocks; divided each litter into two halves by strictly random choice. One half of each litter was assigned to a group to be trained in the old tank, the other half to a group to be trained in the new tanks (where the number in any litter was odd, the odd member went to the new tanks). In this way the 121 young rats of the 21 litters were divided into two groups as nearly as possible alike in constitution. One group of 58 rats trained in the old tank gave an average of errors per rat of 58.20; the other group of 63 rats trained in the new tank¹ gave an average of errors per rat of 37.78. This implies that the

¹ The litter-mates of the two groups were trained parallel in time and in all respects except the tanks.

number of errors made in the new tank must be multiplied by 1.540 (the ratio 58.20/37.78) in order to convert it into the number that would have been made by the same rat or rats if trained in the old tank. In the tables all figures obtained by training in the new tanks have been thus converted into the equivalent figure for the old tanks.

I cannot definitely answer the question—Why is learning in the new tank easier than in the old one? Partly no doubt the slight differences in the dimensions play a part. But the illumination of the gangways and of the walls of the passages leading to them is rather brighter in the new tank. And especially, by an oversight I allowed the curved wall bounding the distal ends of these passages to be painted in vertical stripes of black and white (each about 1 in. in width); and, having begun work with this arrangement, judged it best to continue, as in any case a careful comparison of the new and the old tanks would be necessary. It seems to me probable that this difference of illumination and of its distribution in the old and new tanks is the principal ground of the easier learning in the new tank.

This large difference (expressed by the ratio 154/100) between the old and new tanks was to me quite unexpected. It has important bearing upon any comparison of results obtained in any experiment designed to duplicate or check the results here reported. Dr F. A. E. Crew has conducted experiments along these lines; and I incline to believe that the large differences between the behaviour of rats in his tank and in mine is largely due to differences of illumination, though the dimensions and proportions of parts are also very different.

II. THE CONTINUATION OF THE TANK EXPERIMENT TO THE 34TH GENERATION.

The Second Report describes in Table I the results of training (in the tank) rats of the 13th to the 23rd generation of my T.R. stock. Table II (p. 219 of this article) reproduces this part of Table I and contains in addition the results of training the 24th to the 34th generation of the same stock.

Two features of Table II are of chief interest. First, the increase of facility which at the time of the Second Report had reached its highest degree in the 23rd generation (of which the average number of errors was 25), reaches a further point in the 30th generation (namely 20 errors on the average) and a still further point in the 32nd generation (of which the average number of errors was 17). As was very marked in the bath or water-maze experiment (described in First Report), the advance of facility in the generations 13–34 was not steady and continuous. Rather the advance seems to be like that of the tide on a smooth sandy beach. One sees each wave that reaches a new mark followed by several waves that fail to reach so high; and then comes another which reaches further than all its predecessors. It is possible that, if a considerably larger number of rats had been trained in each generation, these irregularities of the advance would have been smoothed out. However that may be,

the figures of Table II express an undeniable and very considerable increase of facility in the later as compared with the earlier generations tabled.

Table II¹.

Generation of T.R. stock	No. of rats trained	Average no. of errors per rat	No. of errors made by	
			Best rat	Worst rat
13	23	68 +	30 (1)	90 +
14	10	80	42 (1)	102
15	10	70	39 (1)	96
16	5	73	39 (1)	88
17	11	46	9 (1)	147
18	22	62	15 (1)	142
19	15	47	12 (1)	100
21	34	37	9 (3)	74
22	16	36	6 (3)	89
23	26	25	3 (2)	71
24	14	33	10 (1)	62
25	18	38	14 (1)	78
26	23	43	9 (1)	75
27	32	54	12 (1)	96
28	17	44	13 (1)	90
29	20	50	18 (1)	105
30	11	20	3 (2)	56
31	38	40	3 (2)	100
32	42	17	3 (6)	70
33	24	33	2 (3)	73
34	34	36	2 (3)	88

¹ See footnote, Table IV, p. 224.

Secondly, the column showing the number of errors made by the best (the most quickly learning) rat of each generation is perhaps even more clearly significant of an increase of facility in the successive generations. Whereas in the generations 13-16 no rat made fewer than 30 errors, in each of the generations 30-34 we see two or more rats which make no more than 3 errors apiece.

To this evidence of the figures of Table II must be added the evidence provided by direct observation of the behaviour in the tank of the successive generations.

In the Second Report I described (p. 210) hesitant, tentative, cautious behaviour as characteristic of all or most of the rats of the later generations of the T.R. series, contrasting strongly with the reckless direct rushing to and over the gangways characteristic of all the rats of the control stocks. This tentativeness or caution has increased markedly in the later generations of the third period here reported. For example, in training a litter of 6 T.R.₃₄ (i.e. Tank-rats of the 34th generation), I made on the third day the following note: "One shows *extreme* caution, long loitering, and turns back many times after approaching the gangways (both *B* and *D*).” And on the fourth day: "Two now show similar extreme caution and long loitering; they turn back from gangways many

times, sometimes seizing the support below the wire mesh, holding this timidly with an air of extreme alertness and caution, and then pushing off into the water again." On the eighth day: "Three are now loitering for long periods." And on the tenth day: "One loiters in the water six minutes before taking the dark gangway."

I have similar notes for batches of all the later generations. For example, of one batch of T.R.₃₃ made up from two litters: "Much loitering and turning back from gangways; all very quiet and docile—no rushing, no attempts to escape after emerging from water; all remain quietly on platform every time after landing."

Of one batch of T.R.₃₂ my note runs, on the twelfth day of training: "All through hitherto much hesitation and loitering." And of another batch of T.R.₃₂: "Much loitering and many turnings back from both gangways, both with and without touching them. One rat on sixth immersion of first day of training remains loitering in the mid-chamber near *O* for 10 minutes and is then taken out. One rat displays so much caution that on the seventh day after achieving discrimination (*i.e.* having made 42 escapes by *D* (the dim gangway) on these six days) he still continues to approach *D* very cautiously and slowly on each occasion, sometimes turning back before touching *D*, sometimes landing on *D*, and crouching alertly at the foot of the slope, and then sometimes diving back into the water."

In this last case, as in two or three others of similar extreme caution, when I would gently bring my hand near him in order to lift him from the foot of the gangway on which he crouched, he would dive back into the water. On the first day of training of a litter of T.R.₂₃ my note runs: "All very large, vigorous and lively, but show extreme caution and much turning back from gangways. One of these, after receiving a single shock on the first day of training, loiters in the water, refusing to come out, and is taken out on the point of drowning after 5 minutes." And of another litter of the same group (T.R.₂₃) on the same day (also the first day of training) my note runs: "All rather small and a little lacking in vigour; show great caution and much turning back. One, after a single escape with shock, stays in the water 10 minutes, repeatedly approaching and turning back from both *B* and *D*."

I cannot too confidently insist that I have never observed behaviour in the least approaching to this type of extreme caution in the rats of the control groups. It is difficult to express the degree of this peculiarity of the later T.R. generations in numerical form. But I have made some attempt at this by timing the stay in the water of a number of rats on

several successive immersions. One batch of 5 T.R.₃₂ was timed on three successive immersions on the sixteenth day of training: their average stay in the water before emerging was 39 sec. On the same day 6 control rats of group W.C. 1932 (also on their sixteenth day of training) were similarly timed on three successive immersions and their average stay in the water was 7 sec. A second batch of T.R.₃₂ was similarly timed in the water during four immersions of the third day and stayed in the water on the average 36 sec.

Again of 6 rats of group T.R.₂₇ I have the notes: "On second day of training, average stay in water on four successive immersions was 19 seconds; on fifteenth day of training the average stay in water was 17 seconds." And of 6 control rats trained parallel with the foregoing batch: "On second day of training average stay in water on four successive immersions was 6 seconds; fifth day, they continue to rush out rapidly, average time about 6 seconds; on tenth day, stay in water is minimum, about 3 seconds; on fifteenth day, continue to rush out in minimum time; twenty-fourth day, still rush out in minimum time, two only making slight pause occasionally; definite hesitation first manifested by one of the batch on thirty-fourth day, but this one continues to make errors up to the fifty-third day." And of another batch of 5 T.R.₂₇ my note runs: "On second day of training average stay in water was 25 seconds; on tenth day average stay approximately 3 minutes."

The difference of behaviour between the rats of the later T.R. generations on the one hand and on the other the rats of the various control groups indicated by the foregoing notes is representative and typical. The difference cannot be accurately expressed in any figure, because the stay in the water tends to grow longer as the training proceeds and especially as each rat approaches the critical stage of training at which he achieves discrimination, and, for this reason, the figures representing the average stay in the water on any one day of training are not strictly comparable for any two rats. But the difference is so large and constant that its significance cannot be doubted.

III. THE BEHAVIOUR IN THE TANK OF CONTROL RATS OF VARIOUS STOCKS.

For comparison with Table II the corresponding figures for several groups of control rats of various stocks are presented in Table III.

The rats of groups (1), (3) and (4) were the progeny of batches of rats (generally 10 in number) of the stock from which my T.R. series are derived, newly obtained from the Wistar Institute in the years 1926, 1930 and 1932 respectively. The 10 rats

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of group (2) were themselves imported from the Wistar Institute in 1927 and were of the same stock.

Group (5) is formed by amalgamating groups (1) to (4).

The 16 rats of group (6) were obtained from the colony of the University of North Carolina in 1929.

The 11 rats of group (7) were imported from the colony of the Institute for Research in Animal Breeding of the University of Edinburgh in 1931¹.

Group (8) is formed by amalgamation of the groups (1) to (7).

The 22 rats of group (9) were progeny of the 16th generation of my half-stock *B*, trained in the bath or water-maze as described in the First Report. Their training in the tank was not completed.

The 4 rats of group (10) were obtained from the biological laboratory of Duke University and were of unknown ancestry. The number of errors made by them is so large as to suggest some abnormality. I have therefore not amalgamated them with the other controls.

The 22 rats of group (11) were progeny of the 10 rats of group (2) begotten by the latter after they had been trained in the tank.

Table III. *Control groups in the tank.*

Group	No. of rats in group	Average no. of errors per rat	No. of errors made by	
			Best rat	Worst rat
(1)	17 W.C. (1926)	149 +	132	185 +
(2)	10 W.C. (1927)	164	90	299
(3)	20 W.C. (1930)	117	60	237
(4)	49 W.C. (1932)	102	14	362
(5)	96 W.C.	120	14	352
(6)	16 C.N.C.	102	33	210
(7)	11 S.C.	188	16	282
(8)	123 controls	123	16	299
(9)	22 Bath controls	107 + +	69	129 + +
(10)	4 D.U. controls	234	159	351
(11)	22 W.C. ₂ (1927)	149	66	234

The most significant facts presented by Table III when compared with Table II are: (1) no group of controls gave an average number of errors per rat smaller than 102, whereas no group of Table II gave an average per rat higher than 80, and three groups of this series gave 25, 20 and 17 average of errors per rat; (2) the best performer among all the 171 rats which appear in Table III made 14 errors (and his performance was quite exceptionally good among all controls), whereas among the 149 Tank-rats of the 30th-34th generations, 15 made no more than 3 errors apiece; (3) of the 364 Tank-rats of the 19th-34th generations, one only made more than 100 errors (namely 105), whereas the average number of errors of the 123 controls of group (8) was 123, and of this group a considerable number made more than 200 errors.

¹ I owe the groups (6), (7) and (10) to the kindness of Dr J. F. Dashiell, Dr F. A. E. Crew and Dr B. Cunningham respectively.

It is a matter for regret that (for reasons stated in the First Report, p. 289) I am unable to give a figure expressing the average number of errors made by the 1st generation of the T.R. series (as also of all generations preceding the 14th). I had assumed that rats of the stocks from which my T.R. stock was derived would serve at any time as satisfactory controls. But Table III exhibits the disturbing fact that the groups of controls derived from this stock in the years 1926, 1927, 1930 and 1932 show a diminution in the average number of errors from the year 1927 to 1932. This may, with some probability, be regarded as due to some fortuitously favourable selection of the groups of 1930 and 1932. If we accept this interpretation, we may amalgamate the groups (1) to (4) of Table III, thus obtaining the figure 120 as representing the true average of errors made by rats of the untrained Wistar stock; and may assume that the rats of the first generation of my T.R. series would have made on the average a number of errors not less than 120.

But it is just possible that the falling off in the average number of errors from 1927 to 1932 represents a real change of constitution of the whole stock, an improvement of it (in respect to this particular facility) due to influences whose nature I am unable to suggest. If the ancestral stock may be liable to such changes, it follows that entirely satisfactory control animals in such an experiment as this can be obtained only by dividing the original stock used for the experiment into two halves as nearly alike as possible; keeping and breeding the generations of the two half-stocks side by side, under exactly similar conditions, except that the one half-stock is trained generation by generation, and the other half-stock is untrained; and using as controls for animals of any particular generation of the trained half-stock the animals of the same generation of the untrained half-stock¹.

The rats of my Bath series answer very nearly to this condition, departing from it only in that, while each generation of the T.R. series was trained in the tank, the generations of the Bath series were trained in the water-maze. The 22 Bath controls making up group (9) of Table III are therefore the most satisfactory of all the control groups. They were the progeny of the 16th generation of this half-stock, all 16 generations having been trained in the water-maze, and otherwise treated exactly in the same fashion as the T.R. series. The training of this group in the

¹ Dr J. B. Rhine is now repeating this experiment with observation of this condition, and is recording the performance and the pedigree of each animal trained; an heroic undertaking for any one who is not in a position to give his whole time to research, and especially for one who has other strong research-interests.

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tank was not completed; 14 of the 22 rats of the group showed no indication of discrimination at the time when the training was discontinued; therefore the figure expressing the average number of errors which the rats of this group would have made must be very considerably higher than 107, the average number of errors actually made.

IV. THE CONTINUATION OF THE EXPERIMENT COMBINING
TRAINING IN THE TANK WITH ADVERSE SELECTION.

This section presents the results obtained up to June 1933 by the continuation of the experiment (the early stages of which were described in the Second Report, p. 205) which combines training of successive generations of a stock in the tank (by exactly the same procedure as that applied to the T.R. series, cp. section 1) with strongly adverse selection. Table IV presents the principal results hitherto obtained.

Table IV¹.

Group	No. of rats trained	Average no. of errors per rat	Range of errors	
			Best rat	Worst rat
Control rats unselected				
W.C. (1927)	10	164	90 (1)	299
W.C. ₂ (1927)	22	149	66 (3)	234
W.C. ₃ (1927)	37	144	45 (2)	229
W.C. ₄ (1927)	10	114	42 (1)	170
Control rats adversely selected				
W.C. ₅	5	215	204 (2)	229
W.C. ₆ (W.H.)	18	166	75 (2)	241
W.C. ₇ (W.H.)	11	134	81 (3)	196
W.C. ₈ (W.H.)	19	119	63 (1)	194
W.C. ₉ (W.H.)	14	115	54 (1)	150
W.C. ₁₀ (W.H.)	9	147	69 (1)	231
W.C. ₁₁ (W.H.)	14	139	51 (1)	228
W.C. ₁₂ (W.H.)	7	114	69 (1)	192
W.C. ₁₃ (W.H.)	21	129	81 (1)	198
W.C. ₁₄ (W.H.)	20	81	24 (1)	165
W.C. ₁₅ (W.H.)	13	59	20 (1)	120
W.C. ₁₆ (W.H.)	41	58	14 (1)	144
W.C. ₁₇ (W.H.)	36	70	23 (1)	148
W.C. ₁₈ (W.H.)	65	43	3 (13)	129

¹ This table reproduces and extends to June 1933 the lower part of Table I on p. 205 of the Second Report. The figures in brackets of column 4 represent the numbers of rats giving the numbers of errors stated in the column; this remark applies also to Table II.

The first four groups of Table IV present the history of the derivation of this stock (here called W.C. (W.H.), i.e. Wistar controls (worse half)). The adverse selection was first practised on the 3rd generation of the Wistar controls newly obtained in 1927. Of this 3rd generation 37 rats were trained, giving 144 errors per rat on the average. The 5 worst of these (2 males and 3 females whose average number of errors was 215, and of which the best performer made 204 errors) were selected to be

the progenitors of the W.C. (W.H.) stock. Of each generation as many as possible (i.e., with very few exceptions, all born of the worse half of the parent-generation after training) were trained in the tank. As a rule these were trained litter by litter, each litter making one batch for training together; but where small and large litters occurred, the numbers of each training batch were evened up to, or approximately to, 6, or in some cases more than 6.

After training of each batch was complete, the worse half of each batch was put aside for breeding, and the better half was destroyed. Where a batch contained an odd number of rats, the odd one was put to the breeding group; for, under this halving process, there was constant danger of the numbers of the stock falling too low. This selection-process was repeated with each of the generations of the stock W.C. (W.H.) from W.C.₄ (W.H.) to W.C.₁₆ (W.H.). It amounted very nearly, but not quite, to rejecting the better half of each generation (better in respect of facility in mastering the discrimination problem of the tanks), and in breeding only from the worse half. The selection exercised is thus very strongly adverse in respect of this particular facility.

The more important features of Table IV are:

(1) *The continued improvement of the stock in respect of performance in the tank in spite of the strongly adverse selection practised on each generation*, an improvement, which, although, as in the T.R. series, it proceeds irregularly (some generations falling back below the level of achievement of their parents and grandparents), is represented by the difference between the 166 errors per rat of W.C.₄ (W.H.) and the 43 errors per rat of W.C.₁₆ (W.H.).

(2) As with the T.R. series, the range of errors expressed in the last two columns follows a correspondingly and even more decidedly improving course¹.

(3) The last generation of this series hitherto trained, namely W.C.₁₆ (W.H.), presents features of very special interest. Fortunately, I had been able to work up the numbers of this stock, and had trained 41 rats of W.C.₁₄ (W.H.) and 36 of W.C.₁₅ (W.H.). And when the first batches of W.C.₁₆ (W.H.) proved on training to be of special interest, I made a point of training every rat born in this generation (i.e. all born of the worse half of the 36 rats W.C.₁₅ (W.H.), 65 in all from 11 litters born). The remarkably low number of errors per rat, namely 43, is equalled in interest by the large number of rats which make no more than three errors apiece, namely 13 (some of these probably made only

¹ It should be added that the later generations of this stock manifest the same kind of hesitancy or caution as those of the T.R. stock, though in less extreme degree.

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two or perhaps even only one error, but with my group-training method, it was difficult to be sure of this). In this respect they are greatly superior to the later T.R. generations.

(4) A fourth feature of great interest presented by this group of 65 W.R.₁₆ (W.H.) does not appear in the table, and must here be described. It has been an invariable rule of my procedure to open the training of each batch of rats by immersing them six times in the tank with the shocking-current cut off. On the six immersions of this zero-day the illumination is switched regularly from *A* to *B* (or *B* to *A*) after each immersion; and the number of times the rats emerge by *B* and by *D* is recorded. Readers of my First Report may remember that, in the early stages of the T.R. series, I relied upon this method of testing, and showed that the 26 rats of the 8th generation (T.R.₈) showed before training a slight preference for the dim route, *D*; while 35 control rats of untrained stock show a small but decided preference for the bright route, *B*. One reason for persistence in this preliminary testing without shock on the zero-day of training of each batch was the possibility that this preference of the T.R. stock might grow stronger in later generations. But no such increase was recognized by me until I began to deal with

Table V¹.

Group	<i>D—B</i>	Average no. of errors per rat
21 W.C. ₉ (W.H.)	58—68	139
15 W.C. ₁₁ (W.H.)	39—51	129
21 W.C. ₁₂ (W.H.)	53—73	81
13 W.C. ₁₃ (W.H.)	37—41	59
42 W.C. ₁₄ (W.H.)	122—130	58
36 W.C. ₁₅ (W.H.)	111—105	70
65 W.C. ₁₆ (W.H.)	<u>250—140</u>	43
20 W.C. ₁₈ (B.H. ₁)	57—63	50
34 W.C. ₁₈ (B.H. ₂)	<u>127—77</u>	39
13 T.R. ₂₅	36—42	38
23 T.R. ₂₅	65—73	43
26 T.R. ₂₇	79—77	54
16 T.R. ₂₈	45—51	44
16 T.R. ₂₉	39—57	50
11 T.R. ₂₀	28—38	20
38 T.R. ₂₁	115—113	40
10 T.R. ₂₂	<u>41—19</u>	17
25 T.R. ₃₃	<u>96—54</u>	33
34 T.R. ₃₄	<u>123—81</u>	36
(O.T.) 28 S.C.	81—87	
(N.T.) 29 S.C.	83—91	

¹ In this table I have included only 10 of the 42 T.R.₂₂ of Table II. These 10 rats were of two litters, both of which showed very marked preference for *D* on zero-day. The remaining 32 T.R.₂₂ showed the usual preference for *B*. Of the seven litters of T.R.₂₁, one only showed a similar strong preference for *D*.

the group W.C.₁₈ (W.H.), when it was too marked to be overlooked. With renewed interest in this preference as a possible manifestation of a Lamarckian transmission of the effects of training and of adaptation through effort, I drew up Table V.

Table V shows the number of times the dim and the bright routes (*D* and *B*) were taken by rats of the several groups on the zero-day of training; each rat being immersed six times on that day, the illumination being switched from one side to the other after each immersion, and the shocking-current cut off throughout the course of the six immersions.

Groups of four stocks are included in Table V. In the first section come the groups representing the last seven generations of the adversely selected stock, W.C. (W.H.).

In the second place are two groups representing two generations of a favourably selected stock W.C. (B.H.₂)¹.

In the third section of the table come the last ten generations of the T.R. stock.

In the fourth section are two groups of the Scottish control rats of the 1st-5th generations. It will be noticed, first, that these two control groups show the same small but decided preference for the bright route as was shown by the controls of Wistar stock which figured in the First Report (p. 280)².

The figures of chief interest are underlined; they represent the only instances in which on the zero-day a very large and decided preference is shown for the dim route.

In the case of the stock W.C. (W.H.) there is steady predominance of preference for *B*, up to the generation W.C.₁₅ (W.H.), when a slight preference for *D* appears; and in the following generation W.C.₁₆ (W.H.) the preference for *D* becomes very marked.

In the stock W.C. (B.H.₂) (which derives from W.C.₁₄ (W.H.)), the appearance of the preference for *D* is about equally marked and even more sudden, and occurs in the same generation, namely, W.C.₁₆.

¹ These represent my second attempt to perpetuate a stock favourably selected (the better half of each generation being selected for breeding) and also trained in the tank in each generation. They were derived from W.C.₁₄ (W.H.) by favourable selection and are included in Table V because they show the same sudden variation or mutation as W.C.₁₆ (W.H.).

² These control rats are placed in Table V in two groups: one of 28 rats which were tested in the old tank (indicated by the letters O.T.); and one of 29 rats tested in the new tank. I had been inclined to suspect that in the new tank (in which the illumination was probably a little brighter than that of the old tank) the balance might be inclined a little in favour of the dim route *D*. But the figures of these two groups show that there is no appreciable difference between the old and new tanks in this respect. And this conclusion is borne out by comparison of other groups similarly tested in the two tanks.

Is it too speculative to say that the figures suggest we have here evidence of the sudden appearance of a mutation, a mutation which is the consequence and the effect of the training of many generations in the tank?

After the training of fifteen generations in the tank to the avoidance of *B* (the bright route), all of which generations show on zero-day of training a slight preference for *B* (with one slight exception, namely, W.C.₁₅ (W.H.)), in the following generation (W.C.₁₆) of both stocks a marked preference for *D* is shown on zero-day, a preference so marked that, in both groups, *D* is taken almost twice as often as *B*. And the preference is well marked in each of the twelve training batches which contain the ninety-nine rats concerned (i.e. 99 W.C.₁₆).

In this connection the generation W.C.₁₅ presents points of particular interest in both stocks. Six batches of W.C.₁₅ (W.H.) and five batches of W.C.₁₅ (B.H.₂) were trained, and in both groups occurred a single batch which showed on zero-day a well-marked preference for *D*; namely, on zero-day a batch of 5 W.C.₁₅ (W.H.) gave 19 *B*—11 *D* (these account for the very slight preference for *D* of the whole group of 36 W.C.₁₅ (W.H.)), and a batch of 8 W.C.₁₅ (B.H.₂) gave 30 *B*—18 *D*.

In objection to this interpretation, it may be asked: If the mutation in stocks W.C. (W.H.) and W.C. (B.H.₂) is due to the training of them through 15 generations in the tank, why should not the T.R. stock, which has been trained in the tank for 34 generations, show a similar mutation? The answer is that it does. Table V shows the sudden appearance of the marked preference for *D* in T.R.₃₃ (the generation which gives the lowest average number of errors and the largest number of individuals of very few errors, namely 3 or less) and its persistence in T.R.₃₃ and T.R.₃₄.

Again, it may be asked: If we are here dealing with a mutation affecting all three branches of the trained Wistar stock, which expresses itself in two ways, namely, in preference for *D* on zero-day, and in sudden diminution of the number of errors made in the course of training, why do the two expressions not run exactly parallel in the generations? The answer seems to be that while the preference for the dim route in the untrained rat seems to be the expression of one factor (that in respect of which the mutation here discussed appears in the last generations of all three W.C. stocks, and which seems to be a readiness to avoid *B*, or ease of evocation of aversive impulse by *B*), the number of errors made by any rat (or any group of rats) is the expression of two factors (as pointed out in the Second Report, p. 216), namely (α) the ease of

the evocation of impulse of avoidance, (b) the ease with which discrimination is effected when the aversive impulse has been effectively evoked.

The separateness of these two factors has become increasingly clear to me, especially in observing the last few generations of the T.R. stock. Thus of one batch of T.R.₃₃ I have the note: "Begin well and make increasingly many errors on first five days. Peculiar to this much trained stock." The batch of 5 gave 8 errors on the first day, 20 on the fifth. Another batch of 6 rats made 9 errors on the first day, 23 on the fourth, and did not return to so low a figure as 9 errors until the fifteenth day. This sort of thing does not occur in the control rats or those of early generations of the trained stock.

But there arises the further question: If training of successive generations is the ground or cause of the appearance of the mutation, expressed by the suddenly manifested marked preference for *D* (for the dim route as against the bright route), why does it appear after only 16 generations of training in the two closely allied stocks, W.C. (W.H.) and W.C. (B.H.), and not until after 32 generations of training in the T.R. stock? To this question I can suggest no definite answer. But it is clearly bound up with the wider question: Why does the training effect more rapid improvement of the W.C. (W.H.) stock than of the T.R. stock, *in spite* of the strongly adverse selection practised on the former?

The difference is not very marked in respect of the earlier generations of the T.R. series. The W.C. (W.H.) stock attains the degree of facility expressed by 43 errors per rat in the 16th generation. The T.R. stock attains almost the same degree of facility (namely, 46 errors per rat) in the 17th generation. After that the further advance in facility of the T.R. stock seems very slow, until the mutation occurs in the 32nd generation, and the number of errors per rat jumps down from 40 to 17. The only fact which seems to me possibly to throw light on this slowness of improvement of the later T.R. generation is that these gave some indications of degeneration, indications not observed in my other stocks and due possibly to the very long continued and close inbreeding of the T.R. stock. As such indications I would mention the following:

(1) Many litters decidedly below normal weight, *i.e.* below weight of controls at same age (weighing was usually done at the end of the third month).

(2) Litters became decidedly smaller in numbers.

(3) Abnormalities of behaviour, such as cannibalism and desertion of litter by the mother, occurred disturbingly often, so that I feared it might prove impossible to perpetuate the stock.

(4) In the 33rd generation, a large proportion of the stock became affected with a disorder which I called 'pusy tail.' The tail became swollen and inflamed and in many cases rotted away. I could not ascertain whether the condition was induced by biting of the tail. Although these rats were kept in the same room as several other stocks, no rat of these other stocks showed any trace of this disorder.

It is necessary to consider carefully the question whether there was any change in the external conditions of the rats which could account for the sudden manifestation of the preference for *D* on zero-day.

Two facts seem to rule out this possibility. First, this change was first manifested by the group T.R.₃₂, which was trained in the summer and early autumn of the year 1932, and continues to be manifested by T.R.₃₃ which was trained from October 1932 to February 1933. It does not appear in group W.C.₁₄ (W.H.) which was trained in September and October of 1932, nor in W.C.₁₅ (W.H.) and W.C.₁₅ (B.H.₂) which were trained in December 1932 and January 1933, and first appears in this stock in the groups W.C.₁₆ (W.H.) and W.C.₁₆ (B.H.₂) whose training began late in April 1933. Yet if there had occurred any change of external conditions capable of causing this preference for *D*, all stocks must have been equally subject to its influence, and the change should have appeared at the same date (approximately) in both stocks: for all the rats were kept in the same room and otherwise treated alike.

One important change in the conditions of the experiment was made in the period concerned, namely the change from the old to the new tank. I have stated that the illumination of the new tank is a little brighter than that of the old tank. Is it, then, possible that the preference for *D* is merely due to this factor, or to some other peculiarity of the new tank? It has been already shown by the zero-day figures for the last two groups of Table V that, if the new tank has any such peculiarity favouring the preference for *D*, that influence must be very slight only; for both of the two groups of S.C. (one trained in the old tank, the other in the new tank) show the usual slight zero-day preference for *B*. But in order to clinch the case against this interpretation I add the following facts. Of the rats of the group W.C.₁₄ (W.H.), five batches, including 35 rats, were trained in the new tank, and of these 35 rats the amalgamated zero-day performance was 100 *D*—110 *B*; that is to say, although they were trained in the new tank, the old preference for *B* prevailed to the usual extent; therefore the preference for *D* which appears slightly in the next generation (W.C.₁₅) and very markedly in W.C.₁₆ cannot be attributed to any peculiarity of the new tank.

I suggest then vaguely enough that there occurred in the later generations of the T.R. series some ill-defined degenerative change which was adverse to the appearance of the mutation (which seems to have occurred in the 16th generation of the W.C. (W.H.) stock) and postponed its manifestation to the 32nd generation of the T.R. series. The facts suggest, then, that just as the training process gradually brings about in each rat a change which remains latent until it reaches the degree at which it enables discrimination and complete avoidance of *B*, so also in the stock the training of successive generations brings about gradually a change of constitution which, when it attains a certain degree, suddenly manifests itself as a mutation coincident in nature with the effects of training.

Putting aside this suggestion of a mutation, arising from the figures of Table V, and reverting to Table IV, the main feature is the very large increase of facility in the later generations. As with the T.R. series, the progress (as marked by the fall of the average number of errors made by the rats of successive generations) is not steady, but is broken by lapses to lower levels, which lapses are, however, less pronounced than in that other series. But the main superiority of this series over the T.R. series as evidence of Lamarckian transmission is due to the strongly adverse selection. For this, it will surely be agreed, was sufficiently drastic to rule out the vague possibility that, in spite of my best efforts to avoid any favourable selection, such selection had nevertheless prevailed in the main throughout the series.

V. AN EXPERIMENT COMBINING TRAINING IN THE TANK WITH FAVOURABLE SELECTION.

In order to make still clearer the possibilities of effects of selection and to obtain some measure of what markedly favourable selection can effect, I initiated a series to run parallel with the W.C. (W.H.) series, as follows: Of the 11 rats of the group W.C.₅ (W.H.) the worst six were set aside for breeding the next group of the W.C. (W.H.) series; and the best 5 were interbred, and from their progeny I took at random 6 rats (2 from each of 3 litters) and trained them as group W.C.₆ (B.H.). Of their progeny I took the better half (after training), and from this better half bred a group W.C.₇ (B.H.₁); and continued this process of training combined with strongly favourable selection, until, with the generation W.C.₁₂ (B.H.), the stock unfortunately, in spite of all care, became extinct, *i.e.* the group of the better half of the progeny of W.C.₁₂ (B.H.)

proved absolutely sterile. Table VI states the main results of this procedure in form similar to that of the preceding tables.

Table VI.

Group	No. of rats trained	Average no. of errors per rat	Range of errors	
			Best rat	Worst rat
W.C. ₆ (B.H. ₁)	6	70	33	93
W.C. ₇ (B.H. ₁)	6	110	60	174
W.C. ₈ (B.H. ₁)	5	80	57	111
W.C. ₉ (B.H. ₁)	9	124	60	165
W.C. ₁₀ (B.H. ₁)	10	132	63	195
W.C. ₁₁ (B.H. ₁)	9	34	21	49
W.C. ₁₂ (B.H. ₁)	4	126	—	—

The numbers dealt with in this series are so small (owing to the lack of fertility of the stock culminating in the complete infertility of W.C.₁₂ (B.H.₁)), that I have hesitated to publish them. But they do serve (like the similar series W.C. (B.H.₂)) to illustrate the comparatively small influence of such selection. The extreme irregularities of the average number of errors per rat are no doubt mainly due to the smallness of the numbers trained in each generation, a defect I hope to be able to avoid in the further handling of the stock W.C. (B.H.₂), of which two generations only have been dealt with.

VI. BRIEF ANSWER TO SOME CRITICISMS.

This article is already too long and I propose to postpone to a later report detailed answers to criticisms, but I indicate here in very condensed form the nature of the evidence which seems to me to invalidate the principal criticisms which hitherto have come to my notice.

The most serious criticism is the possibility of unwitting favourable selection in the T.R. series. I submit that this is met by the series in which training is combined with strongly adverse selection.

Secondly, it has been suggested in several quarters that the improvement manifested may be due to parental instruction, example or emotional contagion or other parental influence. This suggestion is completely ruled out of court by each of several lines of evidence as follows.

First, in the case of a number of litters of later T.R. generations, I have substituted a foster-mother of control stock (*i.e.* untrained and of untrained ancestry) and the performance has remained unaffected.

Secondly, I have made more experiments in crossing the highly trained stock with control stock (taking in some cases the father, in others the mother, from the trained stock). The results have been closely parallel to those already reported (Second Report, p. 208); namely, the performance

of the progeny is intermediate between that of the two stocks; that is to say, the factor or factors *X* on which improved performance depends *are transmitted through the father* as well as through the mother¹.

Thirdly, under this interpretation there could be no explanation of the fact that the facility increases through many generations; a single generation of training should give results as well marked as those of many generations of training.

Fourthly, in a number of cases of the T.R. series, I have allowed several rats of a given generation to go untrained and have then trained their progeny; and in every case these offspring of untrained parents have shown very nearly the same degree of facility as their cousins whose parents had been trained; that is, they made the number of errors appropriate to their generation.

A third suggestion has been that the slower rats, since they undergo immersion in the tank more frequently than the quicker rats, are more exposed to the risks of electrocution, and that favourable selection has in this way been at work.

My reply is as follows: I make a note of each rat lost or killed after its training has begun. I find in my last note-book which contains my notes on the last 208 rats dealt with, that of these 208 rats, 1 was mysteriously lost and 3 were electrocuted in the tank, while all the rest, 204, completed their training. This is a fair sample of the whole; and it is obvious that the number lost and killed (2 per cent.) is too small to be of any appreciable influence; especially when it is added that electrocution occurs almost without exception in the very early stages of training (*i.e.* while the rats are quite small) and the quicker rats do not escape this danger period.

A fourth suggestion is that the training process (perhaps by way of the electric shocks) tends to induce sterility, and that this influence would then be more strongly exerted on the slower rats; hence a favourable selection.

My answer is that there is no evidence of such sterilizing influence;

¹ There was one exception to the rule that the performances of the cross-bred progeny is intermediate between those of the parents and very much better than that of the parent of untrained stock, an exception so striking that it deserves special mention here. A female of a group of 4 controls, which had made 234 errors per rat (group (10) of Table III) and of which group the best rat made 159 errors, was crossed with a male of the group T.R.₂₇ whose errors per rat were 54. From this crossing 5 young were born and these 5 cross-breds made only 40 errors per rat. This superiority of the cross-bred progeny to the better of the two parental stocks may, perhaps, be an instance of 'hybrid vigour.' It will be remembered that the highly in-bred T.R. stock from which the male parent derives showed some evidences of lack of vigour.

and many rats that have gone through a very prolonged training have borne large litters; *e.g.* the 5 rats which figure in the last section of Table I (First Report, p. 205) as W.C.₃. All 5 (2 males and 3 females) went through very prolonged training (ranging from 68 to 76 days), yet, when interbred, they promptly produced normal litters.

Lastly, it is only fitting to attempt a reply to the chief criticism made by the President of the International Congress of Psychology when I presented my Second Report before that Congress at Yale University. He asserted that the differences of performance made by rats under such conditions as those of my tank experiment are in no way correlated with, or expressions of, differences of innate constitution¹. It would seem that this view is proper and perhaps inevitable to a mechanist, a logical deduction from the view that all details of the animal's behaviour are nothing but mechanical reflex movements. However it is possible only to those who have no personal experience of work of this sort or who are totally blinded by the mechanistic prejudice.

If this view were true it would follow that all my arrays of figures are mere hocus pocus, arbitrarily chosen to support a prejudice². I would claim that, if my figures were of this origin, if my tables of figures were pure fabrications, I could have made out a very much better case; and that even a small element of fraudulent manipulation of the figures presented in these three reports might conceal some of the weaknesses of the evidence of Lamarckian transmission and greatly strengthen the 'evidence.'

However, in support of the general objectivity of the figures, and of the view that the number of errors made in the tank by any rat, and more especially that the average number of errors made by the rats of any group (and, of course, the larger the group, the more fully is this true), does express, or is correlated with, some innate or constitutional quality of the single animal, or of the group, I add the following facts and considerations.

My colleague Dr Rhine and I have in many cases trained groups of the same stock quite independently and obtained closely similar results. For example, most of the generation T.R.₃₂ came to the age for training in the summer of 1932 while I was in Europe: 32 of these young rats were trained by him in August and made on the average 12 errors per

¹ I have to rely here on my interpretation of his spoken words only, and it may be that I have not truly understood his remarks.

² In fact to put forward this view in criticism of my reports is virtually to say, as Bateson virtually said of Kammerer, "The man is a plain liar."

rat (range of errors 3-32). On my return I trained in November two belated litters of the same generation (T.R.₃₂). The one litter made on the average 14 errors (range of errors 3-35); and the other litter (for some obscure reason) was much inferior, making on the average 32 errors per rat (range of errors 18-46).

Again, in many of these cases of training of two batches of the same stock and generation by two observers independently, the two batches have been constituted by the splitting of two or more litters between the two batches. In one such case the one batch made on the average 64 errors, the other 54 errors. In another such case one batch made on the average 53 errors, while the other made on the average 50 errors. Other similar instances might be cited.

The severest test of this kind is to allow one observer to train a batch without knowledge or suspicion of its origin and affinities and to compare his results with those for the group of similar constitution. This we have done in a number of cases, and always with satisfactory results. For example, I made up a mixed lot from four different stocks, which lot were trained by Dr Rhine in complete ignorance of their origins. It included 5 rats of the group T.R.₃₁. The 38 rats of this group T.R.₃₁ made on the average 40 errors; while the 5 rats of the same group trained by Dr Rhine in ignorance made on the average 53 errors. Since one of the batches of the same group trained by me made on the average 50 errors, the agreement was sufficiently close, especially in view of the fact that other members of this mixed lot made errors ranging from 12 to 141. In another such case Dr Rhine (in ignorance) found an average error per rat of 18, while I found 21. In yet another case the corresponding figures for the two batches were 40 and 39.

We have not been able to conceive any other practicable method better calculated to test the objectivity of our figures.

I venture the opinion that, if the descriptions and figures given in this and the two preceding reports are accepted as truthfully stated and reasonably objective, even the wand of the mathematician may not easily exorcise the spirit of Lamarck¹.

¹ My cordial thanks are due for helpful criticisms, especially to Drs F. A. E. Crew, T. M. Sonneborn, J. B. S. Haldane, V. Hazlitt, W. E. Agar, and, most especially, to Dr J. B. Rhine, who, in addition to many helpful criticisms and suggestions, has given much skilled and laborious collaboration during the last four years.

PUBLICATIONS RECENTLY RECEIVED.

The Energies of Men: Fundamentals of Dynamic Psychology. By WILLIAM McDougall. London: Methuen & Co., Ltd. 1932. Pp. xi + 396. 8s. 6d. net.

Prof. McDougall presents in this volume a shorter account of the main doctrine which he has developed more fully in his *Outline of Psychology* and his *Outline of Abnormal Psychology*. The book is intended as a text-book for an introductory course, and, in particular, it is intended for use by students who will have no time for more than one short course in psychology. Prof. McDougall feels that such students "profit little from being made acquainted with a multitude of facts about the nervous system, the structure of the sense-organs, and the varieties of 'sensation,'" and the book therefore deals mainly with the dynamic aspect of mental life.

The main features of Prof. McDougall's 'hormic' doctrine are well known and it is not necessary to discuss them here. But, in this book, there are two important modifications of the author's previous presentations. One is the substitution of 'propensity' for 'instinct' in the discussion of the native endowment of the higher species. The other is the introduction of a theory of the learning process in which 'foresight' is regarded as the key to many riddles. Both these changes add to the clarity and usefulness of the work.

University of Iowa Studies in Psychology. Nos. xvi and xvii. Psychological Monographs, XLIV, Nos. 2 and 3. Princeton, N.J.: Psychological Review Co. 1933.

The first of these two new monographs contains three studies in "Clinical Psychology," i.e. "The Effect of Pain and Emotional Stimuli and Alcohol upon Pupillary Reflex Activity," by W. R. G. Bender; "Some Neuro-Physiological Sources of Action Current Frequencies," by D. B. Lindsey; and "A Comparative Study of the Achilles and the Patellar Reflex Response Latencies as measured by the Action Current and the Muscle-thickening Methods," by C. L. Nystrom. The second contains "A Statistical Comparison of Psychophysical Methods," by F. E. Linder; "The Rôle of the Concept in Reading Ability," by P. G. Murphy; "The Threshold of Feeling in the Ear in Relation to Artificial Hearing Aids," by S. N. Roger; and "The Eye Movements of Good Readers," by Robert Y. Walker.

The papers are all interesting and of a high order of ability. The decision to produce several monographs in a condensed form in a single issue was all to the good, for all of these contributions have a direct and workmanlike style which adds greatly to their force. Prof. Ruckmick and Dr Lee Travis are to be congratulated upon their direction of work of a high standard of value.

Commission Internationale de l'Éclairage en succession à la Commission Internationale de Photométrie. Huitième Session, Cambridge, Septembre, 1931. *Recueil des Travaux et Compte Rendu des Séances.* Published by the Cambridge University Press for the National Physical Laboratory. 1933. Pp. 694. 20s. net.

This beautifully produced book contains a very large amount of material, much of which is of the greatest practical and theoretical importance. The main practical problems discussed concern street and factory lighting; the use of coloured signals in traffic regulation; cinema lighting; head-lights on motor-cars; and various lighting

questions in connection with aircraft. Many outstanding theoretical points are also discussed, some of them terminological and some experimental. The whole forms an imposing record of the work accomplished and promised by the Commission, and it deserves close study by all psychological students who are concerned in a study of vision.

The Psychological Effects of Oxygen Deprivation (Anoxemia) on Human Behaviour. By ROSS A. McFARLAND. Archives of Psychology, No. 145. New York. 1932. Pp. 135.

This is an admirable monograph containing much that is of great interest to any student of human conduct, and indispensable to any writer who is engaged upon this or on cognate topics. In the first part Dr McFarland gives an extremely lucid though concise account of the observational and experimental work on oxygen loss which preceded his own. In the second part he describes and analyses the experimental results which he obtained from an investigation carried out in the Cambridge Psychological Laboratory in this country. He shows that while simple sensory and motor responses are as a rule not greatly impaired until O_2 percentages are lowered to about 8.87, more complex reactions, like those involved in a choice reaction test, suffer more speedily and radically. Neuro-muscular control begins to be affected fairly early, and with acute O_2 loss impairment of motor control is very great. Loss of memory, narrowing of the "field of attention" and the prevalence of fixed ideas are all noticed at high altitudes. The first effect of O_2 deprivation may be positively stimulating, but longer continued exposure often leads to sleepiness or to emotional outbursts. A very interesting part of the discussion deals with the effect of O_2 in bringing basic personality patterns into unusual prominence. An excellent bibliography is included. Dr McFarland may be congratulated upon the production of a significant and important piece of work, which shows great promise of further development.

An Introduction to the Computation of Statistics. By SHEPHERD DAWSON. London: University of London Press. 1933. Pp. 192. 10s. 6d. net.

This book provides in a convenient form such statistical material as the laboratory worker in the biological sciences is likely to require. Its aim is to avoid as far as possible the more theoretical parts of the subject, and although proofs for most of the formulae are presented, the emphasis lies on the *formulae* rather than on the proofs. This is certainly justifiable, for the appeal that the book makes is, as the author intends, to the practical worker. In the section at the end where all the formulae are gathered together, the practical utility of the book might have been made even greater if the author had given a brief comparison of the relative advantages of the different formulae in different cases. This would have been especially helpful in the case of the numerous correlation coefficients that are discussed. The arrangement of the book does not diverge from the usual form, the five chapters being on Tabulation, Measures of Central Tendencies, Measures of Dispersion, Sampling, and Correlation.

General Principles of Human Reflexology: An Introduction to the Objective Study of Personality. By VLADIMIR MICHAILOVITCH BECHTEREV. Translated by EMMA and WILLIAM MURPHY. London: Jarrolds. 1933. Pp. 467. 21s. net.

Bechterev and his followers have attempted, mainly on the basis of experiments of the conditioned reflex type, to replace subjective psychology by an objective science of reflexology. Their experimental technique differs from that of Pavlov's school in that they have studied conditioned motor responses instead of salivary responses, and have employed human subjects as well as animals. Some idea of the variety of problems in human and animal behaviour which they have attempted to solve, and some idea of their main points of agreement and disagreement with Pavlov's results,

may be obtained from this book; but the descriptions of their experiments given here are too brief to be either clear or convincing. Most of the book deals with theoretical questions, and is on familiar behaviouristic lines. It may be hoped that the translation into English of this general account of Bechterev's views will be followed by the translation of fuller descriptions of some of his researches.

Maternal Behaviour in the Rat. By BERTOLD P. WIESNER and NORAH M. SHEARD. Biological Monographs and Manuals, No. XI. Edinburgh and London: Oliver and Boyd. 1933. Pp. ix + 244. 12s. 6d. net.

The experiments described in this book were made at the Institute of Animal Genetics, at Edinburgh University; and they deal with the relation between maternal behaviour and the somatic concomitants of maternity. The authors have employed, in many of their experiments, a new and interesting technique for measuring variations in the strength of the maternal 'drive'—namely, a measurement of the maximum age of a young rat which the mother rat will retrieve when the young rat is placed outside the nest. This technique is supplemented by careful observation of other components of maternal behaviour, such as suckling. A useful summary of previous work is followed by a detailed description of maternal behaviour of the normal rat in captivity. Experimental tests of the conditions which prolong maternal behaviour beyond its usual duration, of the influence of surgical interference with the reproductive organs, and of the influence of the injection of certain extracts, are then described. It is shown that gonadotropic hormones (e.g. from placental extracts) will induce maternal behaviour even in virgin rats.

The whole of the work, both experimental and theoretical, is a model of scientific caution; and the results are an important addition to our knowledge of animal motivation. But it is a pity that in this book, as in so many others, no summary of the experimental results is given either at the end of each chapter or at the end of the book.

The Psychology of Animals. By F. ALVERDES. International Library of Psychology, etc. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. viii + 156. 9s. net.

This book does not attempt to give a systematic account of the findings of animal psychology, but is, rather, a discussion of the concepts which can be used in interpreting animal behaviour. Dr Alverdes argues that, as we cannot attain absolute knowledge, we have to set up 'fictions' in order to understand the world at all. In dealing with animal behaviour such fictions as that "the end determines the means" and "the whole determines the parts" are often more useful than the fiction of mechanism. Dr Alverdes distinguishes between 'explaining' an animal's behaviour, (e.g. in terms of physiological processes) and 'understanding' it by sympathy; and he thinks that the latter procedure brings us "into a more direct relation to the observed facts." The argument is illustrated by examples from many fields of animal behaviour.

Psychologie der Dieren. By Dr F. J. J. BUYTENDIJK. Haarlem: De Erven Bohn. 1932. 2 vols. Vol. I, pp. 273; Vol. II, pp. 252. 3.80 fr. net.

These two volumes in the Dutch counterpart of "The Home University Library" give an excellent summary of the present state of animal psychology. The chapters on learning, habit formation, and tropisms are especially well written. The author is neither a reflexologist nor a behaviourist and is especially critical of Loeb's tropism theory of behaviour. Therefore a certain amount of anthropomorphizing has crept in as an alternative working hypothesis, but not enough to detract from the scientific value of the book.

Studies in the Dynamics of Behavior. By CALVIN P. STONE, CHESTER W. DARROW, CARNEY LANDIS and LENA L. HEATH. Edited by KARL S. LASHLEY. Chicago: The University of Chicago Press. 1932. Pp. xiv + 332. 27s. 6d. net.

The three papers included in this volume are a part of a wider programme of research into emotional factors in personality and delinquency, which has been organized under the auspices of the Behavior Research Fund and the Institute for Juvenile Research. Prof. Lashley remarks, in the introduction, that "the problems of motivation are not yet clearly defined, and much pioneer work is still to be done. The devising and evaluating of methods of study is now of far greater value than the accumulation of masses of indecisive data." The papers are therefore to be judged as "an adventure in methodology."

Dr Stone, in his study of "Wildness and Savageness in Rats of Different Strains," provides convincing evidence that these traits are heritable, and also shows their modifiability by environmental influences. Dr Darrow and Miss Heath, in "Reaction Tendencies Relating to Personality," study in detail the changes in electrical resistance, blood pressure, etc., of the human body under various conditions, and show that there is a relationship between the nature of these changes and the results of temperament tests. In the third paper, "An Attempt to Measure Emotional Traits in Juvenile Delinquency," Dr Landis fails to find any evidence of a significant relationship between the results of the available laboratory tests of emotional traits, and juvenile delinquency.

Each of the three papers is a useful contribution to the subject with which it deals; but it is difficult to see why such diverse materials should be collected into one volume, or to see that any of the papers involve any such radically new methods of research as Prof. Lashley's introduction might suggest.

Intelligence and Intelligence Tests. By REX KNIGHT. London: Methuen and Co. 1933. Pp. ix + 98. 2s. 6d. net.

This little book belongs to the series of *Methuen's Monographs on Philosophy and Psychology*. It well maintains the high standard already set in this series for lucidity and interest. In an untechnical and yet not over-popular fashion it discusses the Two-Factor Theory, the Nature of Intelligence, Physiological Accounts of Intelligence, Intelligence Tests, Facts about Intelligence and the Uses of Intelligence Tests. There is not much that can be said to be original, and the author owes and acknowledges a great debt to Prof. Spearman. The exposition is, however, lively, independent, nearly always accurate, and undeniably attractive. It is perhaps a pity that the chapter on Physiological Accounts of Intelligence seems to have been written merely to assert that there are no such things. Practically no notice is taken of experimental work bearing upon a possible relation between specific central nervous reactions and certain high level mental processes. But, this apart, the book exactly serves its purpose.

The Method and Technique of Teaching. By PERCIVAL R. COLE. Melbourne: Oxford University Press. 1933. Pp. x + 388. 12s. 6d. net.

This book is intended to serve as a practical guide to the school teacher, and especially to the teacher who has to occupy himself with a number of different subjects. General methods are discussed under the heads of 'stimulative teaching, associative teaching, incentives to learning, varieties of procedure, preparedness, sequences, interest, thoroughness, class government and discipline, questioning and examining.' The various special subjects are then dealt with, numerous illustrations being given. Although the book is a little on the long side, it is uniformly clear and sensible. Dr Cole writes obviously with much first-hand experience, and there must be few school teachers who could not benefit considerably by a perusal of his recommendations.

The Development of Learning in Young Children. By LOUISA C. WAGONER. New York: McGraw-Hill Book Co., Inc. \$2.50.

This book is in part a time-table of the successive acquisition of skills by young children, and in part an attempt to describe the methods and problems of early learning. The time-table is of considerable interest. From her own experience in a nursery school and from the data of other child psychologists, Wagoner composes a cinematographic picture of the child in process of learning control of its body, habits of eating and sleeping, the use of language, co-operation with others and emotional responses. Based on wide observation and experiment, the sequence represents the order of capacity to learn. And Wagoner emphasizes that consideration for the capacity for *learning* is the first factor in successful *training*. As well as serving as a guide for those who deal with children, the juxtaposition of material from many sources bearing on particular stages of development is one more step in remedying the confusing scatter of psychological knowledge.

Methods of learning are perhaps less satisfactorily handled. The first chapters are devoted to a description of types of animal learning, their counterparts in child learning, and a rather sketchy intimation of the ways in which the child is better endowed for learning than the animal. The application of this scheme of learning processes to the material in the body of the book consists merely in calling attention now and then to examples of the processes listed. There is no systematic attempt to show what methods are suited to learning different things, what methods are used at different ages, or how the methods are combined as learning advances. The most interesting theme in the book is "the frequent difference between what the child learns and what the adult thinks he is teaching." To discover how this unintentional teaching occurs—how distastes for food are developed in the very act of teaching the child to eat, or wakefulness induced in trying to send the child to sleep—Wagoner points out that it is necessary to be alive not only to the different types of learning, but also to what aspect of a situation is of importance to the child. Without pressing excessively the significance of these child values, or over-emphasizing their difference from adult values, she insists on a healthy respect for them for the purpose of teaching, and for the sake of the happiness of the period of childhood itself.

Child Upbringing and the New Psychology. By R. A. HOWDEN. Foreword by WILLIAM BROWN. Oxford University Press; London: Humphrey Milford. 1933. Pp. x + 105. 2s. 6d. net.

This is another case in which 'new' has nothing much to do with time, for the psychology of which Mr Howden writes has now attained a respectable age. However, the book is on the whole very straightforward, clear and sensible and will be found useful by parents and teachers. There is perhaps one difficulty. Parents and teachers who are urged to improve their methods of dealing with children have for the most part been brought up in very different ways themselves. They may be made a good deal more uneasy about the probable disasters that await their charges than the children themselves and this may be more than a little bothersome to a normal and healthy child. However, Mr Howden maintains a level balance of mind and his book, unlike some others, should not unduly worry the adult.

Kindheit und Jugend. Von CHARLOTTE BÜHLER. Leipzig: S. Hirzel. 1931. Third, enlarged and revised edition. Pp. xviii + 434. RM. 10.

The third edition of this well-known book differs from the first two in that there is less theoretical speculation—which has been reserved for a separate work—, a more thorough incorporation of the relevant literature into the body of the book, and a chapter on eroticism and sexuality in early childhood. As before, the main theme is the analysis of a number of phases that show themselves during childhood, each phase being quite distinctive and growing organically into the next. These phases are organic in the same sense as that of a tree, which leaves definite rings as traces of its yearly growth.

It is astonishing with what exactitude the descriptive methods are now applied to the problems of child psychology—for the book is very largely based on statistical evaluation of questionnaires and diaries—without, however, in the least losing in interest or vitality. It is of great value to the social psychologist as probably the best example of this kind of method, upon which he has to rely almost entirely. The danger is that the next stage must inevitably lead to the formulation of a theoretical structure as difficult to test directly as is the theoretical structure based on the empirical findings of psycho-analysis. The book gives little information about the development of the sense perceptions in general. Thus the immense field of research on eidetic imagery is dismissed in three pages of almost entirely negative criticism. It seems not unreasonable to expect that accurate experimental research may lead to quicker and more direct methods for analysing or diagnosing the various phases of childhood, even at ages earlier than those reached by diaries or questionnaires. It would add to the value of the book from the student's point of view if in later editions more attention is paid to strictly experimental results.

The Mind of the Child: A Psychoanalytical Study. By CHARLES BAUDOUIN.
Trans. by E. and C. PAUL. London: George Allen and Unwin, Ltd. 1933.
Pp. 282. 10s. net.

This book deals with *Complexes of the Object, Complexes of the Ego, Complexes of Attitude, Relations and Regulations*, and concludes by giving some *Hints to Educators*. The title assigned to the volume is perhaps a trifle wide, for most of the abundant illustrative material concerns children who certainly deviate from the usual by exaggeration if not in other ways as well. The 'mind' that is revealed is a jungle, not of disorder, but of rank and rather unhappy growth. But as long as parents who read this kind of literature are not themselves straightway plunged into dismal anxiety and horrible foreboding, perhaps no great harm, and some good, may be done. The translation reads well.

Le Suicide. Par CHARLES BLONDEL. Strasbourg: Libraire Universitaire d'Alsace. 1933. Pp. 134.

For a long time past French psychologists and psychiatrists have been very much attracted by the study of the incidence and causes of suicide. This is partly because of the interest of the subject-matter, but largely because there has developed about the study a first-class controversy between the statisticians and sociologists, and those who are devoted to more individual studies, the psychiatrists. Among recent writers the sociologists are best represented by Halbwachs' *Les causes du Suicide* (1930) and the psychiatrists by Achille Delmas' *Psycho-pathologie du Suicide* (1932). Now comes Prof. Blondel with this altogether admirable and balanced monograph, in which he criticizes both sides and at the same time recognizes the necessity of both. Blondel has not only used well his great opportunities for original psychiatric observation, but he is also a very excellent psychologist who writes with humour and judgment. In this monograph the difficulties of statistical study are set forth fairly, with great clarity and with sufficient illustration. Similarly the dangers of oneness which may attend purely individual studies are fully recognized. Blondel's conclusion could with advantage be applied in other directions as well: "Espérons qu'en présence d'un objet d'étude qui leur est, pour une large part, commun, sociologie et psychiatrie renonceront à se le disputer âprement et sauront enfin, pour le plus grand profit de la connaissance, se donner mutuellement audience et combiner leur effort."

L'Amour et l'Émotion chez la Femme. Par ANDRÉ BINET. Paris: Félix Alcan. 1933. Pp. ix + 165.

The author of this book is a professor *agréé* in the Faculty of Medicine at Nancy. After a general introduction which connects emotion with all kinds of fundamental and acquired active tendencies, the book describes and discusses characteristic emo-

tions and emotional states connected with love as they may be exhibited from infancy to old age. Although the treatment is mainly in general terms, and the author has a liking for the epigrammatic generalization, it is interesting throughout and sometimes very acute. On the whole he has little patience with Freudian types of interpretation. A very interesting chapter on mystical love expresses some much needed doubts as to whether the sexual character of many accounts of religious exaltation may not be due in the main to limitation of vocabulary. The book is in fact sensible, somewhat conservative, often witty in an attractive manner, and not without humour.

Why War? By ALBERT EINSTEIN and SIGMUND FREUD. An International Series of Open Letters, No. 2. London: George Allen and Unwin, Ltd. 1933. Pp. 57. 6s. net.

Prof. Einstein was invited by the League of Nations and its International Institute of Intellectual Co-operation to invite anybody he chose to a "frank exchange" of views on any problem. He therefore wrote to Freud asking "Why War?" He added that war occurs because small groups want power, but that what puzzles him is why larger groups obediently fit in with this desire. Here Einstein's letter and Freud's reply are printed. Each eminent professor congratulates and agrees with the other. If only all people could be genuinely 'international' war might possibly end. But Freud also thinks that it might not. For "it would seem that any effort to replace brute force by the might of an ideal is, under present circumstances, doomed to fail." He then takes the obvious opportunity to expound again—very lucidly—his well-known view that human actions are the outcome of two sets of instincts, the erotic or constructive and the aggressive or destructive. The latter set are alternatively called the "death instinct." One group cannot get on without the other, so whatever builds up life must also destroy life, and what can we do about it? Freud seems to find his views rather disappointing and he rightly surmises that others may agree with him in this respect. Six shillings seems a lot to be asked to pay for the slender amount of wisdom and psychology that appears in these "open letters."

The Psychology of Consciousness. By C. DALY KING. International Library of Psychology, etc. Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. xv+256. 12s. 6d. net.

Mr Daly King expounds and criticizes a number of current views of consciousness, and argues that psychologists usually deal only with the fringe of the subject, evading the central problems. Dr W. M. Marston's 'psychonic' theory is accepted as an account of the physiological basis of consciousness, but not as a theory of its ultimate nature. Consciousness itself, Mr King argues, must not be confused with thoughts, emotions, or sensations, for all these are objects or contents of which consciousness is aware; "consciousness itself is immediate awareness." Mr King discusses the possibility of increasing the degree of our consciousness, and feels that it is both possible and desirable.

Mr King's criticisms of current theories are written in a lively and amusing way; but his exposition of his own views is often obscure. One does not quite know how to take his remark (on p. 145) that Kant's *Critique of Pure Reason* is "open to the gravest suspicion of prematurity."

The Spirit of Language in Civilization. By KARL VOSSLER. Translated by OSCAR OESER. International Library of Psychology, etc. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. vi + 247. 12s. 6d. net.

The dominant theme of this book is the continual conflict between the two aspects of language, which Vossler calls the magical and the mystic. Philology is at first concerned almost entirely with the former; but as soon as mere description of sounds and historical deduction of roots is abandoned in favour of interpretation and functional

analysis, the student of language is involved in psychological and philosophical concepts, which again can be communicated only by means of language. In spite of his peculiarly flowery style and the difficulties inherent in the subject, Vossler succeeds in presenting a very interesting account of the dual aspect of language and of the reciprocal influence of the one on the other. He traces the part played by both natural and artificial languages in welding together communities, and the changes that take place in the structure of a language when the attitude of a community towards religion or towards its social structure changes.

**Die Rechenfehler, ihre psychologischen Ursachen und ihre Verhütung.* Von Prof. Dr J. SEEMANN. 1931. Pp. 122. RM. 3.20.

This is the best exposition of the causes and prevention of arithmetical and algebraic errors that has come under the reviewer's notice. The part played by causes such as optic, acoustic, motor and kinaesthetic images, perseverations, inhibitions, and various attitudes towards the task and the type of work are clearly and succinctly stated and analysed. With the help of numerous examples the teacher is shown how to develop methods for helping the pupil to overcome the various sources of error.

**Das Problem der Schülerbeurteilung.* Von Prof. Dr ADOLF BRUCKNER. 1931. Pp. 90. RM. 2.20.

The author works out correlations between the rankings of school children for intelligence by each other, by their teachers, and by intelligence tests. For ages 15-17 the correlation between test and scholastic achievements and between test and teacher ranking is very low. This is explained by the unstable state of the adolescent. To be of much use, therefore, intelligence tests have to become tests of individual temperament and character at these ages.

**Körperbildung und Persönlichkeit. Beiträge zur pädagogischen Grundlegung der körperlichen Erziehung.* Von Dr HANS MÖCKELMANN. 1933. Pp. 96. RM. 1.80.

The author gives a clear and interesting account of the differences between the Greek attitude towards gymnastics, and the modern tendency towards specialization in sport. His discussions and recommendations on the subject of physical education are based on the psychology of types, particularly that developed by Jaensch and the Marburg school. He shows that Kretschmer's physical types cannot be used as a basis, since there would seem to be no correlation even between his types and physical achievement, let alone between them and characteristic attitudes of mind, or the results of Jaensch's and Mockelmann's psychological tests of various kinds.

**Eidetik und Schulunterricht.* Von MARGARETE ZIMMERMANN. 1931. Pp. 88. RM. 2.40.

A useful summary of the literature on eidetic imagery.

**Psychoanalyse und Pädagogik.* Von Prof. Dr ERNST SCHNEIDER. 1930. Pp. 72. RM. 2.30.

A straightforward introduction to psychoanalysis for teachers, free from too much theorizing about the nature of the unconscious and the like. The chapter on the analysis of difficult situations in educational practice and their treatment is clear and to the point.

* The books marked with an asterisk are all part of the series "Friedrich Mann's *Pädagogisches Magazin*, Abhandlungen vom Gebiete der Pädagogik und ihrer Hilfswissenschaften." Langensalza: Hermann Beyer und Söhne.

- **Die Grundlagen einer experimentellen Bildungspsychologie.* Von Dr WERNER STRAUB. 1931. Pp. 104. RM. 2.70.

The first part works out a systematic approach to the tasks, problems, and possibilities of an experimental psychology of education. The second part is a summary of contemporary psychologies in Germany, also from the standpoint of systematic theory.

- **Neue Versuche über Denktypen.* Von KARL ODENBACH. 1932. Pp. 84. RM. 3.90.

The author tested some 2000 children with seventeen tests all taken or modified from standard intelligence tests. He analysed the results to show the method adopted by a child in arriving at a solution of a given problem. Nearly 1600 belonged to what he calls the 'katacentric' thinking type; that is, they found a solution not by adhering to formal lines, but in various subjective ways by ranging irregularly and synthetically over the whole of experience. He concludes that there ought to be a far closer interrelation between school subjects, and a good deal of emphasis, especially with young children, on teaching and argument by analogy. Incidentally his results show very clearly how hopeless the task and the attempting of general intelligence tests must remain until the psychology of types in general and of thinking types in particular has become clarified.

- **Psychologie der Oberstufe. Beitrag zur Reform der Bildungsarbeit.* Von OSWALD KROH. 3rd and 4th revised and extended edition. 1933. Pp. 356. RM. 6.60.

This book is a companion volume to the same author's *Psychologie des Grundschulkindes* and an excellent complement to purely psychological works like those of Ch. Buhler. The first part deals with the development of the rational and aesthetic functions before adolescence, the second with adolescence, and the third discusses the whole question of reform in the educational aims and methods of the upper classes in German *Volksschulen*. But because the author, who is Professor of education at Tübingen and an experimental psychologist of note, also has wide experience of school teaching himself, the book is equally valuable to all educational psychologists and teachers who are interested in education as well as in teaching.

- **Mutter und Sohn. Vom Werdegang Vaterloser Halbweisen.* Von Dr KARL CLAUS. 1931. Pp. 104. RM. 3.00.

This is a careful and sympathetic investigation of the problems posed throughout the various phases of school life by the education of boys by their widowed mothers. The author follows Kerschensteiner in classifying mothers into 'active' and 'contemplative' types and analyses the special difficulties created by each in dealing with her sons.

- **Milieu und Selbstbestimmung in der individuellen Entwicklung.* Von Dr WALTER POPP. 1930. Pp. viii + 166. RM. 4.30.

The author regards the mental or psychological environment as being of infinitely greater importance than the physical and in this book studies the reciprocal influences at work between the will and the environment.

- Woman's Periodicity.* By MARY CHADWICK. London: Noel Douglas. 1933. Pp. 228. 6s.

The book begins with a brief anthropological and historical study. It then proceeds to discuss the reactions of children to menstruation phenomena. A third section deals with the symptoms of periodicity in adult life, both normal and pathological, and with

their relation to certain family and other social problems. A final chapter deals with changes consequent upon the final cessation of menstruation. The book will probably serve a useful purpose, though it is rather long and not vastly interesting. The discussion is general in character.

A bibliography is appended.

The Philosophy of Yoga: Containing the Mystery of Spirit and the Way of Eternal Bliss. By ELIZABETH SHARPE. London: Lunnac and Co. 1933. Pp. 55.

The Place of Minds in the World. By SIR WILLIAM MITCHELL. Gifford Lectures at the University of Aberdeen, 1924-6. London: MacMillan and Co. 1933. Pp. xxv + 374. 12s. 6d. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

June 17, 1933. General Meeting at Bethlem Royal Hospital, by invitation of the Governor and Physician Superintendent.

Papers.

- 1 (a) "Measurement of Mental Tension in Psychiatry."
By Wm. Stephenson, M.Sc., Ph.D., Ph.D.
- 1 (b) "Scope in Manic and Melancholic Conditions."
By M. MacKenzie, M.D., M.R.C.P.
- 2 (a) "Measurement of Mental Deterioration."
By Constance A. Simmins, M.A.
- 2 (b) "Significance of Deterioration Measurements."
By D. M. Kapp, B.Sc., M.B., B.S.
- 3 (a) "Measurements of the Speed and Flow of Mental Activity."
By Grace L. Studman.
- 3 (b) "Psychiatric Consideration of the Measurements."
By W. H. de B. Hubert, B.A., M.R.C.S., L.R.C.P.

LECTURES.

- May 2, 1933. "The Backward Child."
By Prof. C. Burt.
- May 23, 1933. "Recent Views on Primitive Mentality."
By Prof. M. Ginsberg.

SECTIONAL MEETINGS.

MEDICAL.

- May 24, 1933. Symposium: "Gynaecology and Psychological Medicine."
Papers by Dr Alick Bourne, Dr Leonard Phillips, and Dr C. P. Blacker. Discussion opened by Dr Marjorie Edwards.
- June 28, 1933. "Medical Psychology or Academic (Normal) Psychology; a Problem in Orientation."
By Dr Edward Glover.

AESTHETICS.

- June 9, 1933. "The 'Significance' of 'Significant Form'."
By Mr John Alford.

AN EXPERIMENTAL STUDY OF MEMORY IN RELATION TO THE THEORY OF REPRESSION.

BY SAUL ROSENZWEIG (*Harvard University*) AND
GWENDOLYN MASON (*Radcliffe College*).

(*From the Psychological Clinic, Harvard University.*)

- I. *Introduction* (pp. 247-9).
- II. *Technique* (pp. 249-51).
- III. *General results* (pp. 251-4).
 - (1) *Recall difference scores* (pp. 251-3).
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- References* (pp. 264-5).

I. INTRODUCTION.

MELTZER⁽³⁾ recently reviewed the experimental literature bearing upon that aspect of the theory of repression which posits a relationship between hedonic tone and conscious memory¹. He concluded that there were no unequivocal results on this problem because investigators have looked for universal laws instead of studying individual differences by adequate

¹ Another important aspect—outside the scope of this paper—involves the hypothesis that what is repressed, though inaccessible to conscious memory, is nevertheless unconsciously preserved.

statistical methods. We dissent from Meltzer's interpretation and suggest that present results are inconclusive mainly because the problem of the relation of hedonic tone to memory has never been clearly defined. Individual differences are always to be expected, but if such differences are construed in terms of the degree to which required general conditions are being satisfied, they in no way conflict with the operation of universal laws. It is, however, necessary to define the conditions under which the operation of a supposed general relationship is to be observed.

Now, in practically every one of the experimental studies reviewed by Meltzer, the experimenter has either worked with a sort of hedonic tone that is unrelated to the theory of repression, or has failed to develop under laboratory control the experiences which are subsequently to be tested for recall. Sometimes both mistakes have been made.

Whether or not there is an ultimate difference between conative and non-conative hedonic tone, there is certainly such a difference on the phenomenal level. The pleasantness or unpleasantness associated, for example, with sensory stimulation is not the same as the hedonic tone associated with conative activity, striving to succeed. In non-conative hedonic tone the state of the total organism is less fully involved than in conative hedonic tone. There is also a difference in psychological direction of reference. Sensory pleasures and pains seem to be due to outside stimulation and not to be directly related to the individual's sense of responsibility. But conative hedonic tone derives from the success or failure of a subjective purpose and is thus more closely connected with the individual's self-esteem.

If, with this distinction in mind, we turn to the Freudian theory of repression (1), we find that it is the *conative* type of hedonic tone which is there supposed to be significantly related to memory. More specifically, repression is supposed to operate for *negative* (unpleasant) hedonic tone of the conative type and, more specifically still, for such negative hedonic tone as results when a need is frustrated because of *conflict with the individual's desire to preserve his self-respect* (super-ego). These conditions, it need scarcely be said, must be satisfied by any experiment that is to be regarded as a test of the theory of repression.

As a matter of fact, however, very few such experiments have been done. These few were, moreover, faulty in technique, because the experiences measured in them for recall were not developed in the laboratory but occurred in the everyday life of the subjects outside (cf. (3)). So far as we know, there have been practically no experiments in which the experiences measured for recall were both of the type required

by the theory of repression and, at the same time, products of laboratory control.

One or two minor exceptions should be noted. An experiment by Koch⁽²⁾, in which good and bad grades on examination papers were recalled by students after an interval of time, very nearly fulfilled the required conditions. Though the results of this study were not so interpreted by the investigator, they seem to us to support the Freudian theory. Zeigarnik⁽⁶⁾ worked on the recall of finished and unfinished tasks and found that the latter were much more frequently recalled than the former. Although the tasks were not in general presented as a test of personal merit, when certain of them which had, for one reason or another, been so taken were interrupted, evidence of repression incidentally appeared (⁽⁶⁾, p. 77).

In the experiment we are about to report an attempt was made to confirm the indications of the work of Koch and Zeigarnik. The experiences to be recalled by the subjects were calculated to satisfy the conditions described above as necessary for the testing of the Freudian theory, and these experiences were produced in the laboratory. Because of obvious limitations, however, even this experiment must be taken as nothing more than a preliminary study with very large probable errors.

II. TECHNIQUE.

The subjects were 40 children—25 boys and 15 girls—ranging in age from 5 years and 6 months to 14 years and 8 months. All of them were inmates of the New England Peabody Home for Crippled Children in Newton, Massachusetts¹.

To arouse a genuine need in the subjects, the experiment was conducted as a contest. The children were informed by one of their teachers that they were to be given a test to determine how well they could do puzzles and that a prize would be awarded to the one who did best. We then had as incentives both the challenge presented by the puzzles and a lively spirit of competition for the prize.

The subjects were individually given a series of jig-saw picture puzzles to solve. Each of these puzzles consisted of from four to eight pieces of wood which, when properly assembled, formed a board one foot

¹ This opportunity may be taken to extend our thanks to Mrs N. S. Smith, Miss Vivian Jewett, and Miss Anita Daniels of the Peabody Home for their kind interest and co-operation, and to Dr H. A. Murray of the Psychological Clinic for many helpful suggestions. We wish also gratefully to acknowledge a grant from the Committee on Industrial Physiology of Harvard University that made this research possible.

square and one-half inch thick, on one face of which the picture of some common object was represented in burnt lines. The objects depicted were as follows: Butterfly, Clock, Boat, Bed, Swan, Cannon, Telephone, Horse, Automobile, Plant, Bottle, Flag, Engine, Grapes, Map, Lamp, Coat, House, Book and Man. Since the difficulty of the puzzles varied, it was possible to make a selection appropriate to the age and ability of each child.

On their arrival in the experimental room the subjects were given a set of uniform instructions about the contest and the procedure of the examination. It was especially impressed upon them that varying amounts of time were allowed for the solution of the different puzzles according to their difficulty. They were then given a card on which was a miniature reproduction of one of the puzzles in its completed form to study for half a minute. After this they received the corresponding puzzle to solve, together with a piece of heavy cardboard of the same size as the completed puzzle, on which the blocks had to be assembled. A period of one hour was allowed for each child and as many puzzles were given as could be done in that time. Some of the subjects did as few as four puzzles while others did as many as fourteen in the given time.

The experimenter allowed each subject to succeed in half the total number of puzzles attempted, and caused him to fail in half. (In only one case was it found impossible to arrange an equal number of successes and failures and here there were six of the former and four of the latter.) To the subject, who was not aware of this arrangement, the results obtained seemed dependent upon his ability to perform the tasks. When a subject was not allowed to complete a task, he was stopped just after or just before he had assembled half the number of pieces in the puzzle. It was assumed that at this stage in his work he would be definitely interested in the solution and so really experience frustration. At the same time he would not, as a rule, be far enough through the solution to complete the puzzle mentally. After a puzzle was successfully completed, in order to impress the fact of success, the experimenter made a remark such as "That's good"; and after failure he would say something like: "You didn't do that one so well." A one-minute interval was then allowed to pass in conversation with the subject about the puzzle just done.

An attempt was made to pair the puzzles given to each subject so that the time spent on any puzzle successfully completed would be just equal to that spent on some puzzle not completed. In fact, however, it was not always possible to do this and at the same time control the amount of work done and, when necessary, we retained the latter condition at the expense of the former.

After about forty-five minutes, an interval of one minute was allowed, following which these instructions were given in a conversational tone: "Now, I want you to tell me the names of all the picture puzzles I asked you to do." The experimenter then recorded everything said by the subject and spoke only after the latter had been silent for two minutes. The subject was then asked, "Can you remember any more?" If the answer was "No," the recall was regarded as ended. The following instructions were then given: "Next, I will read the list of all the puzzles you did. As I read the name of each puzzle, I want you to tell me whether you liked it, whether you didn't like it, or whether you do not remember whether you liked it or not." After this followed the instructions: "Now, I will read the list again so that you can tell me whether you didn't finish the puzzle I name, whether you did finish it, or whether you do not remember." The subjects were finally asked: (a) "Did you enjoy doing these puzzles?"; (b) "Do you feel that you did the puzzles well?"

The children were then urged to promise that they would not tell anyone what they had done in the experimental room. The teachers had also made it clear to them that they were under no circumstances to discuss among themselves what went on during the test. These precautions had to be taken in order to ensure a controlled recollection of the puzzles.

A record was kept of all behavioural manifestations of excitement during the experiment.

III. GENERAL RESULTS.

(1) *Recall difference scores.*

Table I summarizes the results of the experiment. The general result was as follows: Letting *C* represent a successful puzzle and *X* an unsuccessful puzzle, 16 subjects recalled more *C*'s than *X*'s, 13 recalled fewer *C*'s than *X*'s, 9 recalled an equal number of *C*'s and *X*'s, and 2 recalled none of the puzzles. One of those who recalled *C*'s and *X*'s equally recalled all of the puzzles. We shall later enquire in more detail into the individual characteristics of each of the chief recall groups just mentioned.

Throughout the remainder of this paper we shall speak of 'recall difference scores' and shall compute these by means of the formula

$$\frac{C's \text{ recalled}}{C's \text{ given}} - \frac{X's \text{ recalled}}{X's \text{ given}}$$

The recall difference score will be expressed as a percentage. If more *C*'s

Table I. *Summary of experimental data.*

	Name	C.A.	M.A.	R.C.	C : X	R.D.	P.R.
1	G. L.	5-6	5-5	O	3:3	0	+2
2	T. M.	5-9	5-3	—	2:2	+50	0
3	M. McI.	6-0	4-2	O	2:2	0	-2
4	W. T.	6-2	4-3	O	2:2	0	-1
5	A. Pe.	6-2	5-8	O	3:3	0	+1
6	N. McD.	6-6	4-2	O	2:2	0	-1
7	E. K.	6-7	5-9	O	2:2	+50	+1
8	O. D.	6-9	6-5	O	4:4	-50	+1
9	B. L.	7-2	6-10	O	3:3	0	-1
10	W. D.	7-2	8-0	O	4:4	-50	+2
11	W. G.	7-8	6-11	O	4:4	0	-2
12	L. H.	7-10	7-4	X	6:4	+25	+1
13	L. A.	8-0	8-1	X	5:5	-20	+3
14	E. M.	8-3	7-11	O	3:3	+33	-2
15	L. C.	8-3	6-9	O	4:4	+75	+2
16	M. C.	8-4	8-0	O	5:5	-20	+2
17	W. S.	8-5	8-9	—	4:4	0	+1
18	F. F.	8-7	8-4	X	5:5	+40	+1
19	B. W.	8-8	8-3	X	5:5	+20	+2
20	R. B.	8-9	8-0	O	5:5	-20	-2
21	J. B.	8-10	8-5	O	6:6	+16	-1
22	M. R.	9-1	8-2	O	6:6	-17	-1
23	F. Ca.	9-2	8-5	O	4:4	0	+1
24	M. J.	9-8	6-9	O	4:4	-50	-2
25	W. A.	9-10	9-1	X	6:6	+50	-2
26	A. G.	10-0	9-11	X	7:7	0	+3
27	F. Co.	10-5	10-11	X	7:7	+14	+2
28	O. M.	10-7	8-8	X	7:7	-29	+2
29	P. H.	10-7	8-1	O	5:5	-20	+1
30	M. M.	11-0	9-0	—	5:5	-20	+2
31	R. P.	11-2	8-2	O	6:6	0	+1
32	A. D.	11-2	12-1	X	7:7	+14	+2
33	S. A.	11-2	8-11	X	7:7	-57	-2
34	F. D.	12-0	11-2	X	7:7	-14	+2
35	A. Pa.	13-2	11-1	X	7:7	+29	+3
36	A. O.	13-10	8-5	X	7:7	-14	+3
37	W. W.	14-0	13-2	X	7:7	+14	+1
38	W. Y.	14-4	11-11	X	5:5	+40	+3
39	N. A.	14-4	12-2	X	7:7	+14	+2
40	C. R.	14-8	11-9	X	6:6	+17	+2

C.A. = Chronological age.

M.A. = Mental age.

R.C. = Repetition choice.

C : X = Successful puzzles attempted in comparison with unsuccessful.

R.D. = Recall difference = $\frac{C's \text{ recalled}}{C's \text{ given}} - \frac{X's \text{ recalled}}{X's \text{ given}}$ (expressed as per cent.).

P.R. = Pride rating.

than X 's were recalled, the score is positive; if more X 's than C 's, the score is negative; if an equal number of C 's and X 's, the score is 0. The group of subjects who recalled more C 's than X 's will thus be called the 'positive' group; those who recalled fewer C 's than X 's will be called the 'negative' group; those who remembered an equal number of C 's and X 's will be called the 'neutral' group.

The order in which the successful and unsuccessful puzzles were recalled might be expected to be of some significance. Our results, however, revealed nothing in this connection that is worth reporting.

(2) *Reports on the outcome of the tasks.*

The subjects were asked to state, as they heard the names of the puzzles, whether they had or had not finished them. These reports should enable us to determine whether the impressions of the outcome of their previous attempts had become distorted in the course of time. It is true that the data which we are about to discuss might be regarded as a record of the actual impression made upon the subjects as they attempted each of the puzzles. The present reports would then indicate whether the subjects had subjectively, even if not objectively, solved the puzzles, and whether they had failed to solve the puzzles subjectively even if they had solved them objectively. However, the children were specifically asked to report the *actual test result* for each puzzle. It is, moreover, unlikely that there were many subjective completions in the absence of objective ones. We therefore consider that the present results represent the distortion through time of the experiences originally registered.

There was a tendency for puzzles to be reported as finished more often than as unfinished, though the subjects made as many unfinished as finished attempts. Twenty of the subjects described more of the problems as finished than as unfinished, 15 reported as many unfinished as finished, while only 5 described more as unfinished than as finished. Considering the 51 errors only, 14 were *C*-puzzles mistaken for *X*, whereas 37 were *X*-puzzles mistaken for *C*. Of the 18 cases in which the subjects reported that they did not know whether they had or had not finished the puzzle, 11 were *X*, 7 were *C*. We might expect that the positive recall group would show more distortion in favour of *C* than would the negative group, and this expectation proves correct. Of the 16 who recalled *C* better, 10 reported that they had finished more than they had not, 1 reported the opposite, and 5 reported that they had finished as many as they had not finished; of those who recalled *X* better, 8 reported having finished more than not, 1 the opposite, and 4 evenly. Of those who recalled *C* and *X* evenly, 4 reported having finished more than not, 3 the opposite, and 4 evenly. All three groups distorted the results in favour of *C*, but the neutral group remembered the results of their work with least distortion.

(3) *Reports on hedonic tone.*

On the basis of other work (4, 5) we should expect that, on the whole, the unfinished puzzles were unpleasant. In the case of the present experiment the situation in this respect is not perfectly clear. Each puzzle had a pictorial representation and this may have influenced affectivity. Also, the reports on hedonic tone were made some time after the affective experiences had occurred, and in the meantime the subjects had been trying to recall the puzzles and had stated whether or not they had finished them. Further, the tasks themselves followed in a constant succession, so that one experience might have obscured the others in recollection. Nevertheless, there was a greater tendency to call the *X*-puzzles unpleasant than to call the *C*-puzzles unpleasant, and, conversely, there was a more marked tendency to call the *C*-puzzles pleasant than to call the *X*-puzzles pleasant. The figures for these tendencies are as follows: of the 189 *X*-puzzles given, 123 were called pleasant and 66 unpleasant; but of the 191 *C*-puzzles given, 167 were called pleasant while only 24 were called unpleasant. There was, however, a general tendency to call all of the puzzles pleasant. This may be accounted for by the facts that more *C*-puzzles than *X*-puzzles were recalled and that a good many puzzles which had actually been *X* were recalled as *C*. Furthermore, the puzzles were, on the whole, fun to the children, and so would be called pleasant rather than unpleasant in case of doubt.

IV. INTERPRETATIONS.

(1) *Repetition choice.*

In attempting to relate the results on recall to the rest of the data we have obtained, the first thing we must consider is the manner in which the present results compare with those on the same subjects in a previous experiment (5). It was there found that the younger subjects consistently chose to repeat puzzles in which they had previously had success, whereas the older ones chose to repeat puzzles in which they had previously experienced failure. This difference in repetition choice correlated with a difference in teachers' ratings on the trait of pride, in which the younger children had a markedly lower average than the older ones. It was suggested by way of interpretation that the older children, because prouder, were more sensitive to failure and strove for self-vindication, whereas the younger children were not wounded by failure and so

ignored it. Since the same subjects¹ participated in the present experiment, a comparison is possible.

Those subjects who preferred to repeat the *C*-puzzle in the previous experiment tended to recall the *X*-puzzles in this and those who preferred to repeat the *X*-puzzle tended to recall the *C*-puzzles. Of the 11 subjects in the repeat-*C* group who recalled differentially, 7 recalled the *X*-puzzles better, while only 4 recalled the *C*-puzzles better; on the other hand, of the 16 subjects in the repeat-*X* group who recalled differentially, only 5 recalled *X*'s better, while 11 recalled *C*'s better. Further, a much greater proportion of subjects repeating *C* than of those repeating *X* recalled evenly. There is thus a clear opposition between the way in which those repeating *C* and those repeating *X* recalled *C*'s and *X*'s: of the former group, a few recalled *C*'s better, more recalled *X*'s better, while the greatest number recalled evenly; of the latter, the greatest number recalled *C*'s better, a lesser number recalled *X*'s better, while the smallest number recalled evenly.

(2) *Chronological age.*

Following up the meaning of this opposition, we begin to suspect that the same factors which made for choice of *C* for repetition made also for absence of preference or preference for *X* in recall; conversely, the same factors which made for choice of *X* for repetition made for preference for *C* in recall. In this connection chronological age might seem to have been a factor of some importance. But analysis does not completely support this impression, for if the 20 younger subjects are compared with the 20 older ones, it is found that in the former group, 7 recalled the *C*'s better, 8 recalled evenly, and 5 recalled the *X*'s better; while in the latter, 9 recalled the *C*'s better, 3 recalled evenly, and 8 recalled the *X*'s better. It thus seems characteristic of the younger group to recall evenly, and of the older to recall *C*'s better; in neither group was there a plurality of subjects recalling *X*'s better. Fig. 1 shows this graphically. If we take the recall difference grouping as our basis of comparison, we find that the median age for the positive group was 9 years and 4 months, for the neutral group 7 years and 2 months, and for the negative group 9 years and 8 months. In general, then, the recall differences cannot be explained by reference to the factor of age taken by itself. Only in connection with repetition choice, which is itself clearly age-linked, does difference in age

¹ In this comparison only thirty-seven cases can be considered, for three subjects who participated in the present experiment did not take part in the experiment on repetition choice.

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throw light upon recall scores. However, we should not expect age to operate directly, but only in combination with other factors that appear, as a rule, at certain ages. We must therefore investigate these underlying factors.

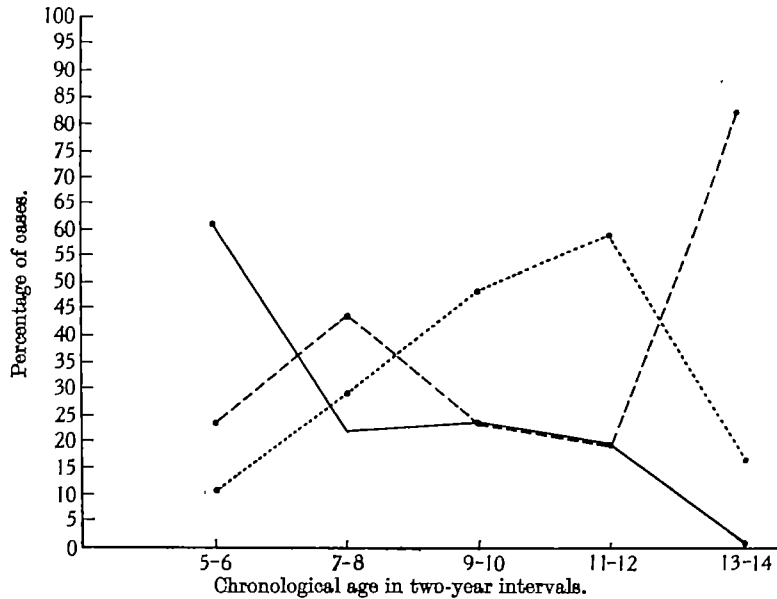


Fig. 1. Relation of chronological age to the recall of successes and failures.
 — = 0 R.D. score, ---- = + R.D. score, ... = - R.D. score.

(3) *Mental age.*

We gave the children an intelligence test—in most cases the Kuhlmann-Anderson and in the remaining ones the Stanford-Binet¹. Fig. 2 shows the relationship of mental age to type of recall. The median M.A. for the positive recall group was 105 months, for the negative group 97 months, and for the neutral recall group 82 months. This seems to indicate that those subjects who were oldest mentally remembered the C's better and that those who were youngest mentally recalled evenly, while those of intermediate mental age recalled the X's better.

¹ It should be noted that the median I.Q. for all the subjects was 92, which is slightly over than normal. This may, however, simply indicate that these crippled children had not done as much school work as is usual for normal children of their ages, in which case little importance should be attached to this slight I.Q. deviation. In other respects the fact that the children were crippled does not seem to have made any noteworthy difference in the experiment.

(4) *Pride.*

The subjects were rated by their teachers for the trait of 'pride.' 'Pride' was defined as "Desire to stand well with the group, and pleasure in one's own achievement." For all the cases in the positive recall group, the average rating for pride was $+1.06$; for all the cases in the negative recall group the average rating was $+0.85$, and for the neutral group it was $+0.18$. Thus, those subjects who showed a preference for *C* in recall had a pride rating which was distinctly higher than that of those who recalled the *X*'s better or recalled evenly. The trend in respect to pride for the three groups ran parallel with that in respect to mental age.

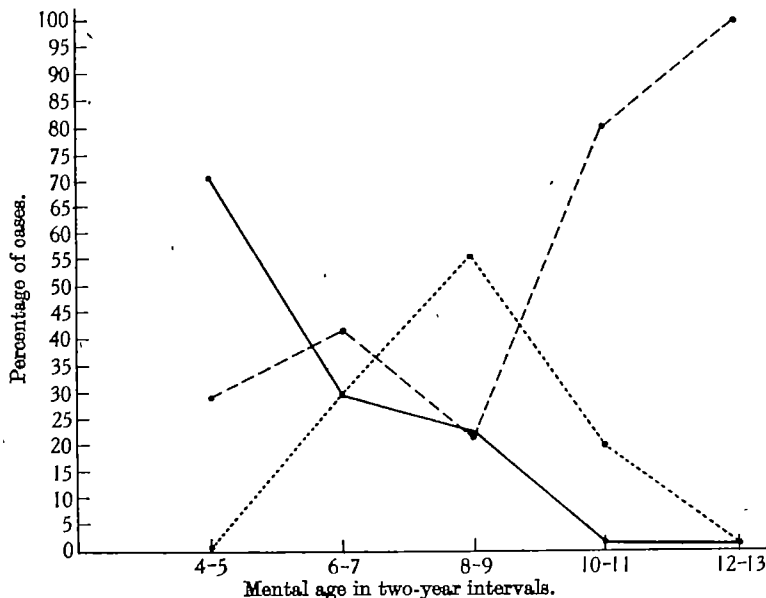


Fig. 2. Relation of mental age to the recall of successes and failures.
 — = 0 R.D. score, ---- = + R.D. score, ... = - R.D. score.

If we take as our basis of comparison the ratings given to the subjects, we get a similar result. Of the 27 subjects who were given a positive rating for pride, 12 recalled the *C*'s better, 9 recalled the *X*'s better, and 6 recalled the *C*'s and *X*'s evenly; while of the 12 who were given a negative pride rating, 3 recalled the *C*'s better, 4 recalled the *X*'s better, and 5 recalled the *C*'s and *X*'s evenly. (One subject was rated 0.) We may then conclude that a preference for *C* in recall tends to be associated with a relatively higher rating for pride, while a preference for *X* in recall tends to be associated with a relatively lower rating for pride; and, similarly, a

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positive rating for pride is more often associated with a preference for *C* in recall than is a negative rating¹.

In this connection we may consider the replies of the subjects to the question, asked them at the end of the test, whether they thought they had done well. Though a comparison of these answers with the results on recall produced nothing of significance, there was a marked relationship between the type of answer given and the relative chronological age of the subjects. Table II shows this. If the forty subjects are divided into three groups according to chronological age such that the first group comprises

Table II. *Answers to the question*
"Do you feel that you did the puzzles well?"

	Youngest subjects	Intermediate subjects	Oldest subjects
Positive answers	8	5	3
Indeterminate answers	4	7	3
Negative answers	1	2	7

the 13 youngest, the next the 14 of intermediate age, and the last the 13 oldest, it is found that in the first group, positive answers, in the second, indeterminate answers, and in the last, negative answers predominated. Thus, self-confidence was the characteristic note of the youngest group, uncertainty or compromise of the intermediate group, and self-condemnation of the oldest group.

(5) *Conclusion.*

What is the probable theoretical significance of the facts disclosed by the foregoing analysis? It would seem to be that, given an individual of sufficient intellectual maturity and a commensurate measure of pride, experiences that are unpleasant because they wound self-respect—perhaps it should be added in a *social* situation—are, other things being equal, less apt to be remembered than experiences that are gratifying to the ego. This is in keeping with the Freudian theory of repression.

¹ It should be noted that a second set of pride ratings for all the children, obtained at a later date, agreed very poorly with those on which the calculations just reported were based. This second set of ratings was the work of the teacher in general charge of the children, who had originally rated only the group directly under her tutelage, while the remainder of the children were rated by the teacher in direct charge of them. On the whole, the first ratings are probably more reliable, since they were made at the time of the experiment and by the teachers in direct charge of the children, but the second ones (which were made without any reference to the first) have the merit of having been made by one person and consequently with a common standard of grading throughout. The disagreement of the two sets of ratings indicates that the reliability of the results on pride is not very great.

In our experiment, at the most 16 of the 40 subjects satisfied these conditions. The 11 very young subjects with neutral recall difference scores seem to have been indifferent to the test situation, so that there was no basis for preferring *C*'s or *X*'s in recall. Their low average mental age and pride rating bear out this interpretation. The group of 13 subjects with the negative recall difference scores had a higher mental age and pride rating, but they were presumably not enough wounded for repression to operate. Their reaction was, however, very definite and consistent. It is as if they took a more objective attitude toward the test, regarding the puzzles as so many external obstacles to be overcome. Their need for mastery of the environment was aroused, but failure to satisfy it did not wound their egos or arouse feelings of inferiority. If anything, feelings of protest and perseverance were awakened by failure and these brought about a preference for *X*'s in recall. The fact that the unsuccessful puzzles were *unfinished* may also have created a perseverative tendency, such as probably prevailed in the experiment of Zeigarnik⁽⁶⁾, making for recall of the *X*'s. Such a perseverative tendency must have been present in the case of the positive recall group, too, but there the competing repressive tendency was so strong that the perseverative one was overpowered. In the case of the negative group the attitude of protest co-operated, rather than competed, with the perseverative tendency, and so the *X*'s were better recalled.

It may, of course, be that we have overlooked important factors in our analysis. For example, the degree of excitement experienced by our subjects may have been important. Perhaps it was excitement rather than pride that in the first instance produced our recall difference scores, pride merely being the factor that produced excitement. We have insufficient data to determine whether this was the case¹. Similarly, we cannot decide about other possible factors.

V. POSSIBLE ARTIFACTS.

An analysis of some possible experimental artifacts will help us to evaluate the reliability of the results already presented.

(1) *Mental scores.*

Since the competitive element was very strong throughout the experiment, it may be that the preference shown in recall for *C*-puzzles or *X*-puzzles was the result of a conscious effort made by the subjects to keep score. However, even if the subjects made such an attempt, would they not simply have counted up the puzzles on which

¹ No noteworthy sex differences appeared in this research.

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they were succeeding or failing, without trying to remember the names of the puzzles? And even if scores were kept in terms of the names of the puzzles, we should still have to ask why certain children remembered more successes while others remembered more failures when all of them had as many of the former as of the latter. The possibility that the subjects kept a mental score does not therefore seem to invalidate the results on memory.

(2) *Assortment of puzzles.*

Since the subjects did not all have the same assortment of puzzles to solve, the differences in memory may have been due to the differences in the puzzles attempted. Ideally, the same puzzles would have been given equally often both as *C* and as *X*, to both the positive and negative recall groups. Actually, this was impossible because of the age and other differences of the subjects. But we can look in our protocols for instances that did fulfil these ideal conditions and see if the results in recall in these instances tally with our general findings. An instance of the results yielded by this procedure is as follows: If the cases of those subjects are taken whose recall difference scores were either + or -40 or more (5 positive and 4 negative cases), it is found that whenever the same puzzle was given the same number of times as *C* or as *X* to each of the two groups (9 cases in all), the positive recall group consistently recalled the puzzle more often when it was *C*, whereas the negative group consistently recalled it better when it was *X*. There was only one exception, in which case both groups recalled the puzzle equally often. For example, the puzzle 'Bottle' was given, in all, 4 times to the 9 subjects in question, twice to the positive group and twice to the negative group and in every case as a *C*-puzzle. In both cases it was recalled by the former group and forgotten by the latter. Other instances were similar to this one, and we may thus be certain that the general difference in recall of the two groups was not simply a matter of the assortment of puzzles they were given to solve.

(3) *Order of puzzles in test.*

Another extraneous factor that may have influenced the recall results is the position of the puzzles in the total series presented. It might be expected that the puzzles done toward the end of the test would be more frequently recalled than those done earlier, and if it happened that the *C*'s or *X*'s were given more frequently toward the end of the test, this might be the main reason why either the *C*'s or *X*'s were better recalled. An analysis of the results, though it does show that position made a difference to the chances of recall of a particular puzzle, does not bear out this suspicion. In general, the puzzles given toward the middle of the test were least well recalled, those given toward the beginning were recalled better, and those given toward the end were recalled best. The representative percentages of puzzles recalled for these three relative positions were 48, 55 and 82, respectively, regardless of success or failure on the puzzles. But if, taking the positions most favourable to recall, we compare the percentage of *C*'s or *X*'s given to the positive and negative recall groups at these positions in the test, we find the following: For the positive group, in the first, second, next to the last, and last positions, respectively, 62, 44, 37 and 44 per cent. of all the puzzles were *C*'s; for the negative group, 100, 31, 54 and 33 per cent. of all the puzzles were *C*'s. This means that on the average the positive recall group had more *X*'s (53 per cent.) in the four most favourable positions for recall while the negative recall group had more *C*'s (55 per

cent.) in the same four positions. It seems clear that if position influenced recall at all unduly, it was *against* rather than *for* the general results we obtained.

A further test of the influence of position as compared with *C* or *X* upon recall is afforded by the following: Let us take the last puzzle given every subject and see whether it was given more often as *C* or as *X*. We find that whereas 28 of the subjects were given an *X*-puzzle last, only 12 were given a *C*-puzzle last, and this result goes with the general result that the *C*'s were more frequently recalled than the *X*'s. Since the last puzzle given was recalled in all but three cases and since, moreover, the difference in preference for *C* or *X* in recall for many of the subjects was only a matter of one puzzle, it is quite clear that the position of the puzzles in the test series could not have been the determining factor in recall. Since the last puzzle was recalled in all but three cases, it would seem to be theoretically advisable to calculate the recall difference scores with the last puzzle omitted. As a check on the scores which we had already obtained, we calculated new ones in this way. This only reinforced the results previously obtained: there were now 19 in the positive recall group, 12 in the neutral group and 9 in the negative group. Moreover, practically all of the factors that we had previously correlated with the other recall difference scores were found to correlate equally well with these new ones.

(4) *Time.*

It has been said that an attempt was made to pair the puzzles so that the time spent on the finished puzzles would be the same as that spent on the unfinished puzzles. With this precaution any difference in the frequency with which the finished and the unfinished puzzles might be recalled could not be attributed to a difference in the amount of time originally spent on them. This attempt, however, for the reason already given (p. 250), was not very successful, and hence it is necessary to determine whether or not certain of the puzzles were recalled while others were forgotten simply because the subject occupied himself with the former for a longer time than he did with the latter.

Now, the average time for all the *C*'s given was 1 min. 45 sec. and for all the *X*'s given 2 min. 25 sec. But since the *C*'s were more frequently recalled than the *X*'s, we may deduce that there was certainly no direct favourable influence of length of working time upon the chances of recall. If there was any influence, it was the very opposite of that which would be expected. Again, if the median time spent on the puzzles is calculated for each subject individually, 23 of the subjects remembered more of the puzzles on which they had spent less than the median amount of time, 6 subjects remembered more of the puzzles with time greater than the median, and 11 remembered an equal number below and above the median. Seventy-one per cent. of all the puzzles below the median were recalled, but only 53 per cent. of those above the median. As a crucial test we may examine the puzzles which consumed extremely long or extremely short amounts of time. This analysis reveals that of the 14 puzzles on which the most time was spent (10 min. or more; longest time 10 min. 8 sec.), 7 were recalled and 7 were forgotten. Of the 7 remembered, 1 was a *C*, the other 6 *X*'s; of the 7 forgotten, all were *X*'s. On the other hand, of the 13 puzzles on which the least time had been spent (20 sec. or less; shortest time 10 sec.), all without exception were recalled. Five of these were *C*'s and 8 *X*'s.

How now shall we account for these paradoxical facts? It seems probable that the

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tasks on which less time was spent were more pleasant or less painful—greater successes, if successes, and smaller failures, if failures. If this was so, we could conclude that the shorter puzzles were better remembered because they were more pleasant, or less painful, and this interpretation accords with our general conclusion on recall.

In order to compare the importance, as regards recall, of success or failure with length of time spent on the problems, an analysis was made of these two factors for each of the three recall groups. Table III shows the percentages of recall for the long and short¹ puzzles and also for the *O*'s and *X*'s in each of the three groups of subjects.

Table III. *Comparison of working time with success as influences in recall.*

Positive recall group.			
Short puzzles recalled:	73 %	<i>O</i> 's recalled:	78 %
Long puzzles recalled:	56 %	<i>X</i> 's recalled:	50 %
Neutral recall group.			
Short puzzles recalled:	70 %	<i>O</i> 's recalled:	60 %
Long puzzles recalled:	47 %	<i>X</i> 's recalled:	60 %
Negative recall group.			
Short puzzles recalled:	68 %	<i>O</i> 's recalled:	48 %
Long puzzles recalled:	57 %	<i>X</i> 's recalled:	76 %

This table clearly shows that while each group remembered the short puzzles better than the long ones, the groups were differentiated with respect to their recall of the *O*'s and *X*'s. Moreover, in the cases of both the positive and negative recall groups the preference for *O*'s or *X*'s was more marked than that for short or long puzzles. Thus, the difference in recall of *O*'s or *X*'s is not to be reduced simply to a preference in memory for short puzzles, even if both preferences are expressions of the same underlying tendency.

A further comparison of the factors of success and time made by considering separately the *O*'s and the *X*'s for each of the three recall groups yields a decided confirmation of this last statement. Table IV shows these results. In the positive recall group, of the total number of *O*'s given, 80 per cent. of those above the median time were recalled, 76 per cent. below the median time were recalled. It may be that where a puzzle was successfully completed it was considered by this group to be more of a success if it had been accomplished after a long struggle. For this reason the long *O*'s would be better remembered than the short ones. This should be borne in mind in comparing success with time in respect to recall; for if long *O*'s were better recalled than short ones in the positive group, it may have been so because the length of time spent upon the puzzles was important for recall only by relation to success or failure.

¹ The short puzzles here, as throughout the remainder of this discussion, are those puzzles which took less than the median time for all the puzzles done by each subject. This means, of course, that what is in absolute measurement a short puzzle for one subject may be a long puzzle for another, but since long and short are meant to be subjective values, the only way of arriving at them is to take the median time for each subject and to record as short puzzles all puzzles falling below that median and, on the other hand, as long puzzles all those falling above that median.

Table IV. *Comparison of short with long successes and failures in respect to recall.*

Positive recall group.		
	Percentage below median time recalled	Percentage above median time recalled
<i>C</i> -puzzles	76	80
<i>X</i> -puzzles	70	31
Neutral recall group.		
	Percentage below median time recalled	Percentage above median time recalled
<i>C</i> -puzzles	69	38
<i>X</i> -puzzles	71	56
Negative recall group.		
	Percentage below median time recalled	Percentage above median time recalled
<i>C</i> -puzzles	62	31
<i>X</i> -puzzles	74	82

In this case the negative group would not have recalled long *C*'s better than short ones, but the opposite—and this is what actually happened. Turning to the *X*'s of the positive recall group, we find that 31 per cent. of those above the median time were recalled and 70 per cent. of those below the median time. Here the shorter puzzles were better remembered. On our hypothesis this would be explained by supposing that the shorter *X*'s were smaller failures than the longer ones, and that since this group of subjects characteristically recalled successes, they therefore recalled the shorter *X*'s better than the longer. In summary, then, if we suppose that the order of subjective success was as follows: *C*'s above the median time, *C*'s below the median time, *X*'s below the median time, *X*'s above the median time, then we have the result for the positive group that they recalled 80 per cent. of the most successful attempts, 76 per cent. of the next most successful attempts, 70 per cent. of the next, and only 31 per cent. of the least successful attempts.

In the case of the negative recall group, of the total number of *C*'s given, only 31 per cent. of those above the median were recalled and 62 per cent. of those below the median. The smaller successes were better remembered than the greater. Of the total number of *X*'s given, however, 74 per cent. of those below the median were recalled and 82 per cent. of those above the median. Those subjects who characteristically remembered their failures better also remembered better the puzzles which were more fully frustrated. If, as before, we suppose that the order of subjective success was as follows: *C*'s above the median time, *C*'s below the median time, *X*'s below the median, *X*'s above the median, then this group remembered only 31 per cent. of the most successful attempts, 62 per cent. of the next most successful attempts, 74 per cent. of the next, and 82 per cent. of the least successful attempts.

In the case of the neutral recall group, of the total number of *C*'s given, 38 per cent. of those above the median and 69 per cent. of those below the median were recalled. That is, this group recalled the smaller successes better than the greater. Of the total number of *X*'s given, 71 per cent. of those below the median and 56 per cent. of those above the median were recalled. That is, this group recalled the smaller failures better

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than the greater. If we suppose the order of subjective success to have been the same as before, this group remembered 38 per cent. of the most successful attempts, 69 per cent. of the next most successful, 71 per cent. of the next, and 56 per cent. of the least successful attempts. There is, then, in this case no consistent connection between recall and the relation of time to success or failure, but this is what we should expect in view of the fact that the neutral group characteristically recalled *C*'s and *X*'s indiscriminately.

The time factor thus in no way invalidates our general results, but rather strengthens them, by lending unforeseen and striking confirmation. The principles that seemed to govern the recall of successes and failures as defined apart from time are reaffirmed by the evidence for recall when success is defined in terms of working time. Moreover, these results are, if valid, of great significance to the general psychology of memory and learning, for they indicate that under certain conditions qualitative factors such as success are of greater importance to recall than quantitative ones such as duration or, in other words, that the law of effect is more fundamental than the law of frequency.

VI. SUMMARY.

In an experiment performed on forty children with the object of determining whether successful or unsuccessful activities are more apt to be remembered, it was found that the subjects could be divided into three groups according as they recalled a greater percentage of the names of the successful than of the unsuccessful tasks (jig-saw puzzles), a lesser percentage of the former than of the latter, or an equal percentage of each. The group that recalled successes better than failures was differentiated from the group that recalled failures better than successes by a more advanced average mental age and a higher average rating for the trait of pride. Some important incidental results of the experiment were (a) that there was a distortion in recalling the outcome—success or failure—of the tasks attempted that ran parallel with the tendencies shown in recalling the names of the tasks; (b) that with increasing age there was an increase in self-critical answers to the question, "Do you feel that you did the puzzles well?" and (c) that the experiences of relatively shorter duration were more readily recalled than those of longer duration, presumably because of the prevailing dynamic conditions. The results of this experiment seem to support certain aspects of the Freudian theory of repression.

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(Manuscript received 26 October, 1932.)

A CHANGE IN THE INTERPRETATION OF THE LAW OF EFFECT

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- I. *The significance of the problem* (p. 266).
- II. *The contrast between the theories of retroflex action and congruity* (pp. 267-71).
- III. *The change in Thorndike's treatment of the problem of selection* (p. 271).
- IV. *The relation of the modified treatment to the theory of congruity* (pp. 272-4).
- V. *Summary* (p. 274).
- References* (pp. 274-5).

I. THE SIGNIFICANCE OF THE PROBLEM.

THIS paper is intended to raise the question: Has not Thorndike in his recent book⁽¹⁾ adopted some aspects of the congruity theory?

The question has considerable importance. Some form of the congruity theory is continually being put forward to replace, interpret or supplement the law of effect. Thorndike alone¹ has consistently refused to countenance either the original congruity theory or any of its derivatives. Consequently his adoption of any aspect of that theory as an interpretation of the law of effect should constitute a major event in the history of the psychology of learning.

Thorndike's adoption of the congruity theory has, however, other and more important significance. Within the *S-R* bond or connectionist system of psychology any explanation of learning in terms of consequences must conform to one of two schemata. One of these is the congruity theory and the other is the theory of retroflex action. One of the most intriguing problems in the psychology of learning is to determine which of these basic theories or schemata is correct. Thorndike's partial adoption of one of them cannot be ignored by those interested in the problem.

¹ McGeech (2) notes only two other 'connectionist' authors (Williams and Stephens) who deny the explanatory value of the conditioned reflex. Pear, Lashley and the Gestalt psychologists are critical, but the two latter are also critical of the entire connectionist position.

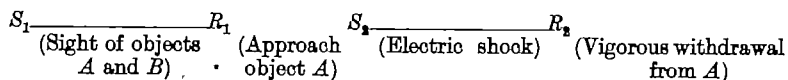
II. THE CONTRAST BETWEEN THE THEORIES OF RETROFLEX ACTION AND CONGRUITY.

In getting at our main question it is proposed to discriminate briefly between the congruity theory and the theory of retroflex action, and then to consider Thorndike's recent position with respect to these two theories. Before doing so, however, it should be made clear that both of these theories attempt to explain the phenomenon of 'back action,' and both subscribe to the *S-R* bond description of behaviour. Consequently the subsequent discussion must be interpreted with the above limitations in mind. That is to say, if the *S-R* bond theory does describe behaviour adequately, and if there is a phenomenon of 'back action,' then the phenomenon would have to be explained in one of two ways.

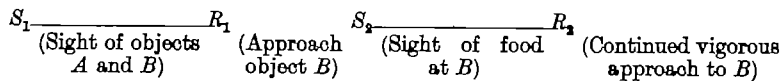
The connectionist system of psychology should need no exposition. The concept of 'back action' may, however, bear restatement. By 'back action' is meant the alleged fact that the conditions which follow an act affect the bond or connection responsible for the act. If the act of stealing some goods is followed by punishment, the tendency or bond underlying the theft may be affected.

The different means of explaining this alleged phenomenon in terms of connectionist psychology are best indicated by an illustration. Suppose that a rat in a discrimination box approached one outlet (*A*), and was then shocked and as a consequence withdrew vigorously from the outlet (*A*). Let us suppose that a second rat approached outlet (*B*), and there saw food and as a result increased the vigour of his approach to (*B*). These facts may be represented by some such diagram as the following:

Rat I:



Rat II:



Let us assume that if the respective treatments are continued rat I will become less likely to approach *A*, and rat II will become more likely to approach *B*. We have already assumed that this change in probability has been brought about by the events which followed R_1 in the case of the two rats respectively. That is the alleged phenomenon of back action. We are now, within the limits of these assumptions, going to discriminate between the respective means by which the theory of

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retroflex action and the theory of congruity explain the phenomenon of back action.

The theory of retroflex action¹ states that if the operation of a bond is followed by 'beneceptive' conditions (Troland), or conditions conducive to the survival of the individual or race (Stephens), the bond will be strengthened; and conversely, if it is followed by 'nocioceptive' conditions (Troland), or conditions conducive to the extinction of the individual or race (Stephens), the bond will be weakened. The theory of retroflex action considers that this strengthening or weakening is brought about by direct retroflex neural action. That is to say, the subsequent conditions act as stimuli for neural impulses which definitely and directly affect the bond in question. Thus, in our diagram, S_2 , in addition to initiating the neural impulse leading to R_2 , may be considered to initiate other and discrete impulses which directly affect the connection S_1-R_1 . We could, with regard to S_2 , differentiate between its primary function (*i.e.* to elicit R_2) and its secondary function (*i.e.* to affect the bond S_1-R_1 which has just operated).

One must remember that under the head of S_2 the theory of retroflex action would include the proprioceptive stimuli arising from R_1 . It is quite fitting that it should do so, since these are set off by conditions which are undoubtedly subsequent to the occurrence of R_1 .

If S_2 can have a stamping-in or secondary function which is potentially separable from its primary function, we can describe the modification of S_1-R_1 purely in terms of the secondary function. Hence the theory of retroflex action attempts to explain the modification of S_1-R_1 entirely in terms of the nature of S_2 . This theory contends that it makes no difference what overt R_2 the S_2 comes to elicit. In order to predict that rat I will come to avoid outlet A , all we need know is that he receives a shock and that receipt of a shock is a biologically harmful condition. In order to make such a prediction we do not need to know what the animal does in response to S_2 . It makes no difference whether he jumps back or forward, or maintains a stoical calm.

The congruity theory on the other hand places the emphasis on R_2 . It maintains that the bond S_1-R_1 will be strengthened if the act which it governs is followed by an act congruous with R_1 . By a congruous act is meant one which reinforces the previous act or shows some biological utilization of that previous act. An incongruous act refers to one which

¹ For the original and most systematic presentation of this theory see Troland (3). A more sketchy and less explicit statement, together with a description of a machine constructed to illustrate this theory, has been given by Stephens (4).

nullifies the previous act. For instance, in the case of our rats, the R_2 of the second rat (continued vigorous approach to B) is held to be congruous with R_1 , while the R_2 of the first rat (vigorous withdrawal from A) is held to be incongruous with the R_1 of that rat.

A more definite idea of the meaning to be attached to the term 'congruity' may be obtained from a brief history of the term. The use of the concept of congruity as an explanation of the selective factor in learning, was first introduced by Hobhouse(5). Hobhouse cited as an illustration the manner in which Lloyd Morgan's chicks learned to avoid eating certain types of caterpillars. He pointed out that if the action of pecking a certain type of caterpillar were followed by the congruous action of swallowing it, then the tendency to peck at that kind of caterpillar would be strengthened, whereas, on the other hand, if that reaction had been followed by the incongruous action of spitting out or ejecting the caterpillar, the tendency to peck at that type of object would be weakened. Hobhouse suggested that this same schema could be applied to other cases of learning.

Since that time several modifications of this general explanation have appeared. Peterson(6) suggested that the selective factor lay in the degree of completeness which the R_1 reached. Thus if the R_1 were continued in uninterrupted fashion, the bond upon which it is based would be strengthened. Let us imagine a rat in a maze at the junction of the true path and a cul-de-sac. The rat chooses the correct path, and as a result progresses in unimpeded fashion toward the goal. If he chooses the cul-de-sac his progress will be cut short and reversed. Peterson regards the progress along the true path as representing a more 'complete' response than does the progress into the cul-de-sac.

Woodworth(7) considers the selective factor to be the incidence of a 'consummatory' response. He takes the term 'consummatory' from Sherrington's distinction between preparatory and consummatory reflexes. In the case of the rat in the maze or discrimination box, Woodworth would consider all the searching movements to be responses preparatory to eating or escape. The preparatory response which is followed by a consummatory response will be strengthened. The others will not be strengthened.

The most popular and ubiquitous derivative of the congruity theory is the attempt to describe selection in learning in terms of the conditioned reflex. It will be seen that the shock (S_2) applied to rat I in our illustration may be regarded as the unconditioned stimulus for withdrawal. According to the schema of Pavlov, if this shock occurs often

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enough in conjunction with the indifferent stimulus (S_1), the latter, which originally elicited R_1 (approach), will come to be the conditioned stimulus for R_2 (withdrawal). That is to say that the bond S_1-R_1 will be changed. In the same way we may consider the sight of food (S_2) which greets rat II to be the unconditioned stimulus for an approach reaction. If this stimulus occurs in conjunction with the indifferent stimulus (S_1) the latter will become a conditioned stimulus for the same reaction. It will be remembered, however, that S_1 originally called forth this approach reaction. Consequently the effect of the conditioning will merely be to make it more likely to call forth the response it originally did elicit. That is to say that, in the case of Rat II, the conditioning process will strengthen the original bond S_1-R_1 . The general position of the conditioned reflex explanation may be stated as follows: If S_1-R_1 is accompanied (actually *followed*) by a dominant action also leading to R_1 , S_1-R_1 will be strengthened, while if S_1-R_1 is followed by a dominant reflex leading to a different R , then S_1-R_1 will be weakened. That is to say, if R_1 and R_2 are identical S_1-R_1 will be strengthened, if not it will be weakened.

In all the above forms of the congruity theory, there is, in describing the selective factor in learning, a consistent use of R_2 . These theories do not attempt to describe selection in terms of S_2 , but in terms of the R_2 which S_2 elicits. The congruity theory or theories contend that in predicting whether S_1-R_1 will be strengthened or weakened, we can ignore S_2 and have regard only for R_2 . If we find that R_2 is a continuance of R_1 , or at least is congruous with R_1 , the bond, S_1-R_1 , will be strengthened. If R_2 is incongruous with respect to R_1 , or a reversal of R_1 , the bond, S_1-R_1 , will be weakened.

The contrast between the theory of retroflex action and the congruity theory and its derivatives is shown most clearly by this difference in the terms in which they describe selection or fixation. In predicting whether S_1-R_1 will be strengthened or weakened, the retroflex theory asks only to know the nature of S_2 , while the congruity theory asks to know only the relation between R_1 and R_2 .

We are concerned here merely with a differentiation between the two ways of describing or predicting the outcomes of learning. We are not trying to determine which description is the more adequate. For a discussion of that problem the reader is referred to other accounts (8, 9, 10, 11). It may not be altogether impertinent, however, to repeat very briefly some of the shortcomings of the congruity theory as seen by one of its opponents. Suppose that in the case of rat I, S_2 had been not a shock but

"food appearing behind rat I." We may now imagine that this would be followed by R_2 withdrawing from A . Consequently while S_2 has been changed, R_2 remains practically the same as far as congruity with R_1 is concerned. Any theory which explains selection entirely in terms of the congruity between R_1 and R_2 would have to predict a weakening of S_1-R_1 . As a matter of fact the rat consistently subjected to this treatment learns to approach A regularly and rapidly. This learning would be predicted by the retroflex theory on account of the biological value of S_2 (food appearing—whether behind the animal or ahead of him).

III. THE CHANGE IN THORNDIKE'S TREATMENT OF THE PROBLEM OF SELECTION.

There has been a general impression that the traditional interpretation of the law of effect attaches chief importance to S_2 . Certainly this was assumed by the proponents of the theory of retroflex action, who considered their theory to be a mere delimitation of the general theory of the law of effect. The assumption was justified in part by the general wording of the law of effect, and in part by Thorndike's spirited attack (8) on the early congruity theory of Hobhouse.

The more recent statement of Thorndike does seem to suggest, however, that the selective agent of the law of effect is located in R_2 . I quote at some length his statement of the action of rewards and punishments (the designating letters are mine):

A. "First, a satisfying after-effect which belongs to a connection can be relied on to strengthen the connection.

B. "Second, an annoying after-effect under the same conditions has no such uniform weakening effect. In certain cases, known by general observation or displayed in experiments such as those of Hoge and Stocking and of Warden and Aylesworth, an annoying after-effect does weaken the tendency which produces it.

C. "Third, when it does so, its method of action is often, perhaps always, indirect. That is, the person or animal is led by the annoying after-effect to do something else to the situation which makes him later less likely to follow the original connection.

D. "Fourth, what he is led to do directly is often (1) either to make a native or acquired response to the particular annoyance in question (as when he responds to annoyance at a certain place by leaving that place, or to annoyance by a certain object by avoiding that object, or to annoyance in the mouth by spitting out the mouth's contents), or (2) to have an idea or other awareness of the undesirability of such and such behaviour....

E. "Fifth, what an animal is led to do directly by an annoyer need not make him less likely to follow the original connection. For example....

F. "The influence upon learning of both satisfiers and annoyers depends upon what they cause the animal to be or do...." ((1), pp. 311-12.)

IV. THE RELATION OF THE MODIFIED TREATMENT
TO THE THEORY OF CONGRUITY.

I will attempt to restate the relevant portions of the quotation in terms of our S_1-R_1 , S_2-R_2 schema.

(1) When an annoyer (S_2) follows a connection (S_1-R_1), its primary function is to elicit a native or acquired response (R_2) (from excerpt D).

(2) This eliciting of R_2 is sometimes, perhaps always, the means by which the annoyer (S_2) influences the original connection (S_1-R_1) (from excerpt C).

(3) This eliciting of R_2 is not bound to influence the bond S_1-R_1 (from excerpts B and E).

(4) The influence upon learning (S_1-R_1) of *any* S_2 is achieved by the R_2 which that S_2 elicits (from excerpt F).

In statements (2) and (4) above, the congruity theory seems unquestionably involved. In statement (2) it seems given as an explanation of what happens "sometimes, perhaps always" in the case of annoyers, while in statement (4) the congruity theory seems to be applied to all effects.

In this connection we should observe that my statement (4) may not be considered a fair paraphrase of excerpt (F). Thorndike says that the influence of effects depends on what they cause the organism to be or do. Now as to the *do*, statement (4) is certainly no distortion. An effect causing an organism to *do* anything must unquestionably be translated as S_2-R_2 in the Thorndike system. An effect causing an animal to *be* something is not, however, so surely translated. The translation will be easier if we re-word the statement so as to drop the *do* part of it (since that has been already discussed), and to turn the infinitive *to be* into a more easily handled substantive. Excerpt (F) could then read, "The influence of effects depends upon the state of being which they bring about in the organism." We have now to determine (a) what state of being is meant, and (b) how the concept of an effect causing a state of being should be translated in terms of our S_2-R_2 schema. It is possible that the state of being may refer to the final outcome of the learning. That is to say, as a result of the effect the organism in our illustration may *be* less (or more) likely to approach A. In that case the statement would be meaningless as it would merely indicate that effects influence learning by influencing learning. If the statement is to have any meaning it must involve some more mediate state of being which in turn and by its nature determines the final outcome of the learning. In view of the Thorndikean system,

one would expect that the state of being involved is that of *being satisfied* or *being annoyed*. Such specific states of being, however, are not deducible from excerpt (F). They are introduced here merely to act as possible illustrations of the mediate state of being which is definitely implied in any meaningful interpretation of excerpt (F).

The fact that we readily consider these states of being to refer to conditions of satisfaction and annoyance, complicates our next task, namely, the determination of how the arousal of this state of being is to be described in S_2 - R_2 terminology¹. If the state of being referred to a physical condition resulting from an effect, as for instance the secretion of saliva following the appearance of food, or the trembling of muscles on hearing bad news, there would be no hesitation in describing it as a response to that effect. In so far, however, as that state of being may, and probably does, refer to an affective condition such as being satisfied or annoyed, there is room for the suggestion that such a condition may be merely the subjective correlate of the effect. That is to say, the annoyance may not be a response to being stuck with a pin, but may be merely the subjective aspect of that experience. We will not try here to settle the quarrel between such a parallelistic interpretation and the interpretation of an interactionist. It seems sufficient to show that the parallelistic position has no place within the Thorndikean system. His position is indubitably indicated by his use of the following expression, taken from an earlier work (12), but typical of many expressions in his later writings: "to the situation 'humble approval, as by admiring glances, from anyone' man *responds* originally by great satisfaction" (italics mine).

In view of the above it does not seem too much to say that when Thorndike speaks of an effect bringing about a state of being, that state of being, whether physical, psychical or both, is considered as a response (R_2) to the effect (S_2). That is to say, statement (4) correctly translates excerpt (F) with regard to both *be* and *do*.

We may say, then, that Thorndike agrees with the proponents of the congruity theory up to the point of locating the selective factor in the R_2 . Does he also agree that the bond is strengthened by a congruous R_2 and weakened by an incongruous R_2 ?

Our question seems to be answered *in part* by the parenthetical portion of excerpt (D). This is taken almost directly from the earlier statements of Hobhouse. Certainly excerpts (C) and (D) say in effect: "Annoyers function by eliciting certain incongruous responses which make the animal less likely to follow the original connection." Conse-

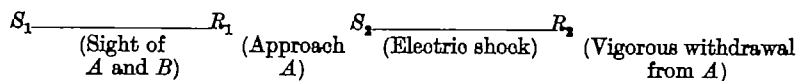
¹ I am very grateful to Prof. H. S. Jennings for some suggestions at this point.

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quently we can attribute to Thorndike a complete adoption of the congruity theory in explaining the influences of annoyers. In explaining satisfiers, on the other hand, he follows the congruity theory to the extent of locating the selective factor in R_2 . Whether he considers that the R_2 produced by a satisfier affects the bond by providing a congruous, reinforcing outlet, or by some other means, is not clear from the excerpts.

V. SUMMARY.

In a typical case of learning such as that illustrated in the following schema:



most 'connectionist' psychologists would agree that any modification of the bond S_1-R_1 would be brought about by the events following the occurrence of R_1 . At this point, however, agreement ceases. The proponents of the theory of retroflex action assert that the selective agent is in the S_2 , while the proponents of the congruity theory maintain that the bond is influenced by the nature of R_2 , or at least by the motion of neural energy toward R_2 . The congruity theory has received its most devastating criticism from Thorndike. This paper holds that many of Thorndike's recent statements show a good deal of dependence on the congruity theory. The paper also refers to some of the limitations of the congruity theory, in view of which its increasing popularity is rather surprising.

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(*Manuscript received 6 March, 1933.*)

THE SPEED FACTOR IN INTELLIGENT REACTIONS.

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I. INTRODUCTORY.

(1) *Problem.*

THE most serious criticisms of group tests of intelligence arise from the fact that most of these tests require the subjects to work against time. Two arguments are commonly involved in these criticisms. First, it is said that such conditions flurried some candidates and so prevent them from doing their best. The second criticism is more serious, namely, that there are subjects who have a greater ability for speed than for accuracy in dealing with test items. Thus the ranking for intelligence based upon a time-limit test is vitiated by non-intelligent factors. In support of this view it is stated that most subjects would score more points in the tests if longer time were allowed, and, therefore, that the limits of their abilities are not reached. The implication is that the scores made in extra time may have little relationship to the original scores. To meet these objections it is desirable to allow subjects to work through a series of problems of increasing difficulty until solutions are no longer forthcoming, little regard being paid to the times taken; and the measure to be used

should be the highest level of difficulty at which success is obtained. Such is the method of Binet, and the measures got by this method are taken to indicate the apparently essential attribute of intelligence, viz., its 'power' or 'level.' However, since group tests are generally constructed to correlate highly with accepted standards, and are exceedingly useful in their present form, they cannot be discarded until the criticisms have been definitely established.

The problem may be considered in two stages. Firstly, to provide direct evidence, the effect of removing the time limits from the tests may be observed. Secondly, to follow up the results of such observations, the existence and nature of a speed ability in intelligent acts, or the more general question of the nature of the time differences in these acts, may be investigated.

(2) *Are intelligence tests invalidated by a time limit?*

The effect of the time limit on the validity of the group test can be examined by allowing subjects to work for an extended period, and then comparing the scores obtained in the longer times with those made in the shorter times. Both sets of scores can also be compared with some independent criterion, e.g. with Binet mental ages, which are obtained under unhurried conditions. Many comparisons of this sort have been made and the results agree in most cases. For example:

(i) May (8) gave the Army Alpha test to a large number of men, and then allowed a further period equal to the standard time for each sub-test. The total scores in standard time correlated with the double-time scores with $r=0.965$, hence it was concluded that "we have no reason to assume that an extension of time limits would have improved the test or have given an opportunity to any individuals materially to alter their ratings."

(ii) Ruch and Koerfh (11) obtained a correlation of $r=0.945 \pm 0.006$ between scores in standard time, and under conditions of no time limit. Highsmith (5) pointed out that this result was spuriously high because the group was formed by combining the highest and lowest deciles of a larger group, and there was thus too large a scatter. But the lower relationships which he obtained by separating the deciles and treating these as separate groups are still unsatisfactory because of the selection.

(iii) Walters (16) found the total scores in five group tests given first in standard time, and then with no time limit, to correlate with $r=0.934 \pm 0.007$. He also compared the scores in standard and in extended time from each of the tests with an independent criterion and found the extended time scores to be correlated to a slightly higher extent in most cases. This was taken by him to indicate the existence of a speed factor, although a rise in the correlation should have been expected, since in extended time there was a longer and hence more reliable test. In any case, when the cores in all five tests were combined, the two sets of scores correlated almost equally with the independent criteria, the standard-time scores being slightly superior.

These and other similar results prove definitely that the differentiation for intelligence by a time-limit test is certainly not affected to a noteworthy extent by the imposition of a time limit. If the rankings for intelligence are not satisfactory, then the fault lies in the matter and not in the method of the test.

(3) *Is there an ability for speed in intelligent reactions?*

From the results just quoted, it must be inferred that there is little sign of an independent ability for speed in intelligent acts. In face of this evidence it may be asked how it is that there is a widespread popular opinion contrary to this finding. Spearman⁽¹²⁾ has mentioned as one source of this belief the fact that some people exhibit a *preference* for speed rather than for accuracy in the uncontrolled situations of everyday life. There is also the tendency of some individuals to work more efficiently than others, with greater 'freedom from load' as Downey⁽³⁾ describes it. Thus in ordinary actions there is a speed factor, a trait which is probably of a temperamental nature, and so the general opinion has some evidence in its support. Nevertheless, although quickness and inaccuracy may characterize the normal actions of some people, this behaviour is apparently overcome in the test situation. Here it seems that the difficulty is eliminated by demanding a uniform attitude of maximal effort, and by keeping accuracy constant. There still remains, however, a very strong defensive perpetuation of the belief in a speed factor. "I could have done better with more time" is the common response of most subjects, especially adults, who have been given a time-limit test.

It is remarkable that many psychologists accept the existence of a speed factor in intelligent reactions. Most text-books, in describing the disadvantages of group tests, give this factor as the serious objection to the tests. Thorndike⁽¹³⁾ regards speed of response as an aspect of intelligence to be measured separately, and some of his workers have procured data which demonstrate its reality as an independent individual difference⁽⁶⁾. How does this apparent contradiction in the evidence arise? Examination shows that there is in fact no disagreement, but only different conceptions of the speed factor. Individual differences are known to exist in the speeds with which *simple* cognitive tasks can be performed, e.g. tasks like cancellation, solving easy problems, substitution tests, etc. These differences are deep-rooted and have been shown to persist when all the subjects have used maximum effort; and after extraneous factors, such as amount of previous training, have been eliminated. It has also been shown that there is a tendency for certain

individuals to be consistently quick in different types of such processes, and these individuals have been described as having a general ability for speed in their 'mental reactions.' Further, this speed ability has been proved to be independent of intelligence level, and must therefore be measured separately. But the effect of such a factor in intelligence tests must be questioned. The speed ability is here conceived as a facility in simple associative processes, and in measuring it, the need for very simple tests is emphasized. If difficult, the times taken to do the tests are not measures of the individual's speed ability because this would be obscured by the difficulty factor. For example, Walters (16), in getting measures of this speed ability, chose the amount done in the tests rather than the amount done correctly because the latter was more indicative of ability than of rate of work. Clearly this speed ability need not be taken into account in measuring intelligence by time-limit tests since these are not simple. The times taken to do them are greatly influenced by their difficulty, and a factor which is effective only in material of negligible difficulty will not affect seriously the measures which the group tests are intended to give. Undoubtedly a speed ability of this type is an important individual difference, but it cannot be considered an objection to the use of time-limit tests.

The quickness to be considered is one affecting typical intelligent processes. One of Webb's (17) subjects said that there are persons who "do not see the point so soon but (they) grasp it more thoroughly." A method of testing the existence of such a characteristic will be similar to that used in the time-limit experiments, i.e. the comparison of the performances in limited time with those in ample time. The previous work is undoubtedly against any appreciable speed factor operating in the tests, but it is desirable to have a more careful examination than the usual group test conditions permit. Bernstein (1), one of the few investigators who has sought for such a factor, used this method with a large number of specially constructed tests. In one series of these ample time was to be given, while in another there was more work to be done within the given time than could be managed by all but the brightest subjects ('leisure' and 'haste' tests respectively). His results showed the two sets of scores to be correlated equally with teachers' estimates of intelligence and of slowness. The crucial measure, the difference between the 'leisure' and 'haste' scores, showed scarcely any relationship to the other measures or estimates, and in particular, was related negligibly to the estimate of slowness. Thus he was led to reject the view he held before starting his work, namely, that there was a special ability for speed in intelligent acts.

Although admirable in principle, Bernstein's experimental conditions do not seem to fulfil his requirements. With his method, "slower subjects could fairly be assumed to do relatively better in the first kind of tests ('leisure' tests) than in the second kind ('haste' tests)." Evidently speed of work was intended to have little influence in the first tests, whereas it was to be a very marked factor in the second series. If the 'leisure' tests were reliable, the subjects should have been able to do all they could in the time given, but it is doubtful whether this was the case. The time allowed for tests of both kinds was 30 sec., there being fewer but more difficult items in the 'leisure' series. The mere knowledge that the time was short would tend to destroy a 'leisurely' atmosphere, and in any case there seems to have been too much to do. For example, in one of the 'leisure' tests, about eighty words had to be read, and then eight words written as completions, and it seems impossible that children of 11-14 years of age could exhaust their responses in the time allowed. If this were so, the two series of tests would not have provided materially different conditions. For the comparison of the two series to have been of value, at least the fast subjects ought to have finished the 'leisure' tests, if the slow subjects were to do relatively better. To test this objection a battery of the 'leisure' tests, from samples given in Bernstein's monograph, were given to a class of normal children. This group had 36 boys and girls of average age 12 years 6 months, the age of many of his subjects. It was found that not one completed a 'leisure' test within the given time. Extra time enabled all of the subjects nearly to double their scores in these tests. Bernstein's data therefore cannot be accepted as satisfactory evidence for, or against, a speed factor in intelligent reactions; and thus material, other than the extended time results, for an answer to this question is lacking.

(4) *Is there a group factor of speed?*

So far we have considered the existence of a speed factor independent of the 'level' factor which is the quality usually implied by the term intelligence. It must be asked, however, whether a speed factor exists independently of intelligence interpreted as the general factor. Spearman (12), using some of Bernstein's results, has shown that there is no such factor, or in his terminology, there is no group factor of speed, the intercorrelations of the scores on the intelligence tests being entirely due to 'g.' Since we have reason to doubt the usefulness of Bernstein's results we must also hold this conclusion in abeyance until more satisfactory data are produced.

(5) *Suggested methods for testing the existence of a speed factor.*

The following methods are suggested for providing data to test the existence of a speed factor in intelligent reactions.

(1) Let a group of subjects solve problems of the type given in intelligence tests, doing these as quickly and as well as they can. The problems must not be too difficult; otherwise only a few of the subjects will succeed in a number of them. Let there be recorded for each subject the time taken to do each problem which is solved correctly, and from the problems given select those which are solved by all, or nearly all, of the group. Since the group will include a range of intelligence, the problems done by all will not be difficult, but they will be much more difficult than simple processes like cancellation, etc. In addition, let there be obtained from each subject a measure of 'level' of intelligence. This will be obtained from similar tasks, but arranged in a series of increasing difficulty. The problems are to be solved under conditions of ample time, the subjects being asked to do as many tasks as they can.

Since the time taken to solve a single problem will not be a reliable measure, let the times for a few problems be combined by suitable statistical procedure. We shall then expect any two such reliable measures to correlate positively by virtue of the 'level' factor, for it will be granted that the time required for a problem will be dependent in part upon this factor. The question is whether an additional factor will be a source of correlation between the time measures, and an answer to this will be obtained from the value of the relationship between the time measures when the influence of the 'level' factor is removed. If this be significantly positive, we shall have proof of an independent ability for speed in intelligent acts.

(2) Another method, similar, but employing conditions more like those in the group test, would be to have the subjects perform group tests of moderate difficulty with maximum speed and accuracy. The scores on these tests could then be interrelated and the 'level' factor, measured separately, could be removed as before by partial correlation.

(6) *The nature of the time differences in intelligent reactions.*

Whether or not a factor of quickness in intelligent reactions is proved to exist, the nature of the time differences requires investigation. If, as the evidence indicates, these prove to be differences in intelligence, then, as Boring(2) has pointed out, their analysis should provide an experimental attack upon the nature of intelligence. Consequently they

have suggested that, if the speeds of constituent parts of the simplest intelligent act be measured, the source of the speed differences must be located. Thus, one process in the series of decreasing complexity from association time, cognition and reaction times, reflex speed, to speed of nervous conduction, will fail to reveal the speed differences. Many investigators have shown that speeds in the first three of these bear little relationship to intelligence or to speed in intelligent acts(9). Travis(14), following Boring's suggestion, related reflex speeds to intelligence and obtained a very high correlation at first, but in a later publication contradicted this result(15). New data showed the absence of relationship, and a re-examination of the original data revealed that "the records of reflex time secured in the former study could not be read with sufficient accuracy to secure reliable results"!

It is not surprising that this analysis of the structure of the intelligent act has failed to reveal the nature of the time differences even in the simplest act, for it removes the intelligent character of the act. With complex problems the approach of Rounds(10) is much more promising. He has suggested that here the means of arriving at a solution would perhaps be the chief factor in the time differences. The speed would be proportional to the number of steps taken in achieving the desired result, and to compare the speeds in these acts with the speeds in simple processes, *e.g.* the reflex latency, would be like comparing the reflex latencies with the times taken by bricklayers to lay one brick, where one individual used three movements, another five, and so on. A difficulty in following up this suggestion is that it is not usually possible to count the steps made in complex intelligent acts, but a method of doing this will be described later.

Perseveration is naturally a factor which would be expected to have some influence on the times. Bernstein, however, found that his measures of perseveration correlated negligibly with the scores in both the 'leisure' and 'haste' tests, and with the estimate of slowness the value was low. But conclusions cannot be too definite on this point as perseveration has not yet been satisfactorily measured.

II. PRESENT INVESTIGATION.

The present investigation was undertaken to elucidate those problems upon which it was thought that evidence either was lacking, or required supplementation. These problems were: (1) the existence of a factor of speed independent of level in intelligent reactions, (2) the existence of a

factor of speed independent of the general factor 'g,' (3) the nature of the time differences in intelligent acts.

(1) *The existence of a factor of speed independent of level in intelligent reactions.*

The methods adopted to obtain the evidence on this question followed the general scheme already suggested. In the first experiment the times taken for several problems were intercorrelated, and these relationships examined when the influence of the level factor was removed. In the second experiment the intercorrelations of tests of moderate difficulty, done at high speed, keeping accuracy constant, were treated similarly.

Exp. I. Subjects: In this experiment the subjects were 170 Borstal youths tested during an investigation undertaken by Prof. Drever in 1930.

Tests and measures. The measures desired were (1) level of intelligence obtained under conditions of ample time; (2) the times taken for a number of problems. The tests used were (a) the Kuhlmann-Anderson group tests of intelligence; (b) the Drever-Collins performance tests.

The Kuhlmann-Anderson tests were given to the subjects as part of Prof. Drever's inquiry, and were used here because of their possible value as a measure of general level of intelligence. The performance tests were chosen chiefly for a reason to be mentioned presently, and also because this series of performance tests could supply both the measures needed, since it included a large number of problems of varying degrees of difficulty. To get the time measures, those tests solved correctly by all, or nearly all, the subjects, were used, and the total score on the test was taken to indicate the level of intelligence. In getting the speed measures, an attitude of maximum speed and accuracy was desired, yet specific instructions to hurry were unsuitable as they would have been apt to make the subject adopt a crude trial and error procedure. The attitude of doing the tests brightly was obtained in practice by asking the subjects to do the tests as well as they could, and by the presence of the experimenter's stop-watch. The tests were thus given under the standard conditions(4), except for the recording of additional data. These were: (1) the time taken for each test solved within the given time limit, and (2) the number of moves made. (The times are not usually required, as credit for any test is given if the solution is obtained within the time allowance, and the number of moves is not credited.)

For the speed measures it was found that nine of the simpler tests could be used. These were divided into three groups, to which they belonged in the scale. The first group had four of the tests from the Kohs

block-design test, the second to the fifth of the series in the Drever-Collins scale; all of these tests use only four blocks. (In this test the subject is required to build a given coloured design with cubes painted in different colours on the six faces.) The second group was made up of three tests of the form-board type, namely, the profile test, where the parts of a dissected profile have to be assembled, and the two well-known tests, the two-figure, and the Healy A, form-boards. A third group comprised the first two of the three cube-construction tests in the scale.

The time taken to do a single short test, being affected by all sorts of factors difficult to control, was a very unreliable measure for our purpose. Variation in effort especially affects the speed in cognitive processes, as Wild's results have shown (18). To reduce these effects the times in each group of tests were combined. This was done by converting actual times into units of semi-interquartile range, as this unit is probably the most suitable for the skewed distributions which the times for any task usually give. As representative of each group the median of the components was used. An example will make the combining method clear. One subject took 10, 20, 30, and 25 sec. in the four block-design tests. On converting to deviation units these became +1.0, +0.8, +0.1, and +1.2, with times shorter than the median taken as positive. The median of these four values, +0.9, was used as this subject's speed measure for this group. The composite measures thus obtained from the three groups of tests were named: (1) speed in blocks; (2) speed in form-boards; and (3) speed in cubes.

An estimate of the reliability of these speed measures was obtained from the intercorrelations of the separate tests within each group by using the Spearman-Brown formula for a test n times as long as the sub-tests, where n is the number of these sub-tests in the composite. These values are shown in Table I.

Table I. *Intercorrelations of the speeds in the separate tests of the three groups from which the composite speed scores are made.*

	Block-design				Form-boards			Cube-construction	
	1	2	3	4	1	2	3	1	2
Sub-test 1	—	0.380	0.368	0.081	—	0.235	0.240	—	0.446
" 2	—	—	0.406	0.451	—	—	0.082	—	—
" 3	—	—	—	0.423	—	—	—	—	—
" 4	—	—	—	—	—	—	—	—	—
Averages			0.352			0.186			0.446
Reliability of composite			0.68			0.41			0.62

[$r=0.250$ is just five times its P.E. for 170 cases.]

Results. The magnitude of the speed factor is to be sought in the values of the partial correlation coefficients between the speed scores when the influence of the 'level' factor is removed. The total correlations between the speed and 'level' measures are given in Table II, while the partial values between the speed measures are in Table III.

Table II. *Total intercorrelations of level and speed measures.*

	1	2	3	4	5
(1) Kuhlmann-Anderson mental age	—	0.700	0.310	0.541	0.465
(2) Performance test score	0.700	—	0.484	0.648	0.609
(3) Speed in blocks	0.310	0.484	—	0.332	0.332
(4) Speed in cubes	0.541	0.648	0.332	—	0.555
(5) Speed in form-boards	0.465	0.609	0.332	0.555	—

[$r=0.250$ is just five times its P.E. for 170 cases.]

Table III. *Partial intercorrelations of the speed measures, with (a) Performance Test score constant (top right half); (b) Kuhlmann-Anderson score constant (bottom left, with values in brackets).*

	1	2	3
(1) Speed in blocks	—	0.0275	0.0547
(2) Speed in cubes	(0.205)	—	0.266
(3) Speed in form-boards	(0.223)	(0.406)	—

[$r=0.250$ is just five times its P.E. for 170 cases.]

Conclusions from these results. Before drawing conclusions from these results some preliminary factors have to be considered. Firstly, the possible influence of chronological age on the correlations has to be tested. With the performance test score, the Kuhlmann-Anderson mental age, and the median of all the nine speed tests, the Pearson coefficients of correlation are $r = +0.02$, $r = +0.05$, and $r = -0.05$ respectively. Secondly, it has been mentioned that the speed measures give a skewed distribution. The correlation ratios in typical cases, however, showed the regression to be rectilinear, and the Pearson coefficients are retained. Lastly, it must be shown that the factor which has been removed from the speed correlations by the statistical treatment does not include speed. Since previous work shows that the group-test ranking is not influenced by the time limit set, the score on the group test was taken as a measure free from any speed factor. But it is better to use a measure taken under conditions which allow responses to be made within an ample time limit. The time allowances in the performance tests were of this character. Time limits must be set to any test in practice, but they can be chosen so that they afford ample time for an intelligent response. In Kohs' account

of his block-design test⁽⁷⁾ (his time limits are used in the Drever-Collins scale), he says, "the time limit set for each design is about one minute longer than the time within which a correct response may reasonably be expected." For the subjects of this group the actual distributions of the times are significant. With the easier tests of the scale the scatter lies well within the given limit, which is about five times the semi-interquartile range from the median. In the more difficult tests the peak of the distribution lies within the allowance. And when it is remembered that the actual times allowed for assembling about a dozen pieces according to the pattern given are from about 3-5 min., it will be granted that the total score on these tests is primarily one of level of intelligence.

The average of the total correlations between the speed measures in Table II is $r=0.406$. When level of intelligence, as measured by the performance test, is removed, the average of the partial relationships between the speed measures falls to $r=0.116$. Of the three values in the partial relationships, two are negligibly small, while one is statistically significant, that between the speeds in cubes and form-boards. But there is no reason to suppose that the cubes and form-boards tests would have a source of correlation which is not shared by the blocks test. Actually the latter test resembles the cubes test much more than does the form-board test, so the average of the partial correlations is probably the more important measure. This value is insignificant statistically, yet, since all of the relationships are positive, it cannot be neglected. We must therefore conclude that under the conditions described the time taken to solve problems of moderate difficulty is almost entirely determined by the level of the intelligence. It is nevertheless possible that a few individuals who tend to solve problems more quickly than others do not possess as great a level of intelligence. However, it is to be remembered that the conditions of the tests, without specific instructions to hurry, possibly allowed 'speed preferences' to exert a small effect.

Exp. II. An attempt to eliminate the effect of speed preferences was made by instructing a group of subjects to do a test of moderate difficulty with the greatest possible speed and accuracy.

For this sixty-six students attending the Psychology class were used. The work chosen was the National Intelligence test—Scale A—Form 2. Time limits were experimentally determined so as to prevent any subject from finishing any of the sub-tests. The measure of 'level' was subsequently obtained from a more difficult test, the Otis Advanced Examination.

Results. As before, the results desired are the partial correlations

between the speeded tests when the 'level' factor is removed. Table IV gives the total intercorrelations of the five sub-tests in the National test and the 'level' measure (Otis score), while Table V gives the values when the 'level' factor is made constant.

Table IV. *Total intercorrelations of the National sub-tests and Otis score.*

	1	2	3	4	5	6
(1) Otis score	—	0.32	0.32	0.24	0.44	0.43
(2) National 1	0.32	—	0.41	0.25	0.41	0.00
(3) " 2	0.32	0.41	—	0.34	0.42	0.10
(4) " 3	0.24	0.25	0.34	—	0.42	-0.02
(5) " 4	0.44	0.41	0.42	0.42	—	0.16
(6) " 5	0.43	0.00	0.10	-0.02	0.16	—

Average correlation between speed tests, $r=0.25$.

Average correlation between Otis score and speed tests, $r=0.35$.

[$r=0.36$ is just five times its P.E. for 66 cases.]

Table V. *Partial intercorrelations of the National sub-tests with level (Otis score) rendered constant.*

	1	2	3	4	5
(1) National 1	—	0.34	0.18	0.31	-0.16
(2) " 2	—	—	0.28	0.33	-0.05
(3) " 3	—	—	—	0.35	-0.14
(4) " 4	—	—	—	—	-0.04
(5) " 5	—	—	—	—	—

Average of the partial correlations, $r=0.14$.

[$r=0.36$ is just five times its P.E. for 66 cases.]

Conclusions from these results. We have again taken a measure from a time-limit group test as being one primarily of 'level.' The average of the total relationships between the speeded tests turns out to be less than that between these tests and the Otis score, which shows that any speed factor operating in the speed tests is of limited extent. But again we find that the speed measures retain a positive average intercorrelation when the 'level' factor is removed.

It will be seen that this average is decreased considerably by the negative correlations between sub-test 5 and the others. This test is a substitution test, and hence differs from the others in being of a lower order of intellectual difficulty. Nevertheless, it correlates as highly with the Otis score as any of the other sub-tests, and this cannot mean that the Otis score involves more 'pure speed' than these easier tests do, but rather that the substitution test is not such a 'pure speed' test as it would seem to be. There is thus no reason for excluding it from the group of sub-tests, but, since its relationships are so exceptional, the averages

of the total and partial intercorrelations of the speed tests when it is left out are given below.

Average of the total intercorrelations of the first four sub-tests in the National test, $r=0.38$.

Same for the partial correlations, $r=0.30$.

The difference between these two values is not so great as that between the corresponding values when the substitution test is included. In that case the average falls from $r=0.25$ to $r=0.14$. Hence, though not significant statistically, the constancy of sign in the partial relationships, leaving out the substitution test, demonstrates the existence of a factor of speed in these simple problems.

Exp. IIa. The effect of reducing the difficulty still further was observed. The subjects were the same students, with the addition of another twenty. The tests were:

- (1) Cancellation of letters, of the usual type of this test.
- (2) Addition of groups of three single digits.
- (3) Forty simple arithmetical problems.
- (4) Forty simple sentence completions from Thorndike's OAVD series (13).
- (5) Passages of easy directions from the Woodworth-Wells Association tests.

The scores from (1) and (2) were taken as the number of correct responses within the time allowed. In the other tests the subjects completed the given work and recorded the times they had taken to do each part.

Individual timing while administering the test to a group was secured by the following method. Three sets of numbers were made, large enough to be seen at a distance of several yards, each set having the numbers from 0 to 9. The three bundles were fastened to the vertical edge of the table on the experimenter's platform, and folded back upon the top of the table. The bundles were arranged so that by releasing the first sheet in each bundle the number 999 was exposed. This three-figure number was then changed by a unit every 2 sec. by releasing a sheet from the appropriate bundle. [Starting with the largest number first facilitated subtraction to get the times taken by the subject.] The subjects were asked to work as quickly yet as accurately as possible. At certain places in the tests they had to enter the time figure exposed when they had reached that point. These intermediate entries enabled a check to be kept upon the reliability of the recording. In two of the tests the times

for the two halves correlated $r=0.60$, and $r=0.71$, which is satisfactory when it is remembered that the times include variability in performance as well as possible errors in recording.

Accuracy was kept constant in the first two tests by counting correct responses only, while in the other tests the few subjects making errors were penalized. This was done by adding to the total time the average time per item of the test, one for each error, *e.g.* if the time required were 80 sec. for the forty-item test and two errors were made, the time for scoring was taken as 84 sec.

Results and conclusions. The intercorrelations of these tests and the Otis scores are shown in Tables VI and VII.

Table VI. *Total correlations of the speeds in tests of low difficulty and Otis score.*

Test	1	2	3	4	5	6
(1) Otis score	—	0.41	0.20	0.44	0.03	0.24
(2) Cancellation	0.41	—	0.22	0.43	0.11	0.08
(3) Addition	0.20	0.22	—	0.28	0.20	0.41
(4) Directions	0.44	0.43	0.28	—	0.19	0.33
(5) Sentences	0.03	0.11	0.20	0.19	—	0.36
(6) Arithmetic	0.24	0.08	0.41	0.33	0.36	—

Average correlation between Otis score and speeds, $r=0.27$.

Average correlation between speeds, $r=0.26$.

[$r=0.33$ is just five times its P.E. for 86 subjects.]

Table VII. *Partial correlations between these speed measures when level (Otis score) is constant.*

Test	1	2	3	4	5
(1) Cancellation	—	0.16	0.31	0.11	-0.02
(2) Addition	—	—	0.21	0.19	0.38
(3) Directions	—	—	—	0.20	0.25
(4) Sentences	—	—	—	—	0.36
(5) Arithmetic	—	—	—	—	—

Average partial correlation between the speeds, $r=0.21$.

[$r=0.33$ is just five times its P.E. for 86 subjects.]

It will be seen that the interrelationships of the speeds in tests of low difficulty are almost independent of the 'level' factor, as measured by the Otis score. When this is removed, the average of the partial relationships is found to be very little changed, actually from $r=0.26$ to $r=0.21$. Hence an ability for speed in these tests is quite apparent.

The general conclusions on the question of the existence of a speed ability independent of 'level' in intelligent acts are, therefore, that such an ability exists to a noteworthy extent only in problems of very low difficulty. When these are more complicated, as in the performance tests, the times show almost complete dependence upon the 'level' factor. The similarity between the results from the National test and

the tests using simple tasks suggests that the former may have been of a rather low degree of difficulty for our purpose. Watching the subjects showed that they could do the tests without deliberation, the responses being made almost as fast as they could be entered. This was not the case with the performance tests used as speed measures. Thus the absence of the speed factor in the group tests used to measure 'level' for any particular group is apparently due to the fact that the limits afforded more than sufficient time for all the easy items to be done.

(2) *The existence of a speed factor independent of 'g.'*

Tables II, IV and VII have been tested for the existence of specific correlation by applying the tetrad difference criterion. These tables include speed measures, and measures primarily indicative of level of intelligence. This latter quality we know to be highly saturated with 'g,' and we also know that the correlations between the speed measures are partly determined by this factor. Therefore, if there is no factor of speed independent of 'g,' i.e. no group factor of speed, the tetrad differences from each table should diverge from zero only to an extent possible by the operation of chance errors. Spearman has given a formula for calculating the probable error of the distribution of tetrads from a table of intercorrelations (¹²), Form 16a) and by comparing this value of the expected divergence with that actually obtained, it is made clear whether there is a group factor as well as the general factor producing correlation between the tests.

Results and conclusions. The observed and theoretical dispersions of the tetrads are given in Table VIII.

Table VIII. *The fifteen tetrad differences for each of Tables II, IV and VII.*

Table II	IV	VII
0.007	0.008	0.011
0.008	0.008	0.012
0.013	0.012	0.023
0.018	0.025	0.023
0.026	0.025	0.029
0.030	0.033	0.034
0.030	0.033	0.035
0.031	0.037	0.044
0.036	0.040	0.051
0.043	0.041	0.055
0.054	0.048	0.063
0.059	0.050	0.103
0.061	0.063	0.119
0.067	0.066	0.140
0.089	0.070	0.154
Observed median 0.031	0.037	0.044
P.E. theoretical 0.027	0.039	0.039

The close agreement of the observed P.E. of the tetrads in each table with that expected from chance errors proves definitely the absence of a group factor of speed in tests of any degree of difficulty. Not one tetrad difference is more than four times the estimated P.E. As Spearman says (12), "‘*g*’ has shown itself to measure a factor both in goodness and in speed, of cognitive process." This result is in line with his conception of ‘*g*’ as mental energy, or, as Boring describes it (2), mental power, the ability to do intellectual work against time.

(3) *The nature of the time differences in intelligent reactions.*

Having found no appreciable factor of speed in reactions of moderate difficulty, the question now arises as to the rôle of intelligence in producing these time differences. It has already been mentioned that the latter are probably due to the number of steps taken in solving the problems, a good intelligence exhibiting fewer steps of a trial and error kind than a poorer one. In testing this there is difficulty in analysing the time differences, as it is not usually possible to count the number of steps taken in solving problems. However, it was thought that performance tests might offer a method of doing this, hence their selection for the comparison of times in the earlier section. In these tests, the steps are made with concrete material and they can be recorded with a fair degree of objectivity. Further, the same critical steps have to be made by all subjects. If the correlation between the number of steps and the time taken should prove to be high, the conclusion will be that individuals do not make steps at very different rates, and that the time taken for a problem is due to the number of steps the subject has to make. On the other hand, should this relation prove to be low, it will show that individuals may make the same number of steps in quite different times, and hence analysis of the time differences would have to proceed within the single step. Of course it is not suggested that all the steps are made with the pieces of the test, for good subjects will probably make many of these imaginally; but previous experience with these tests made it clear that many subjects make most of their solutions by trial with the actual pieces.

Exp. III. The subjects and tests were those described in Exp. I except for the extra data (the numbers of moves made by each subject). A move was taken to be any change of position of a block or blocks (sometimes two blocks were moved at once and this was counted as one move), when this was apparently related to the solution. Random movements of the parts when the subject was not looking at the whole were

not counted as moves. Since there were several observers it was thought advisable to check the recording of the moves. This was done by comparing the median number of moves in each test for the groups of subjects tested by each observer. These were found to be sufficiently alike to include all the cases in one group.

Results. The correlations between moves and times for three groups of tests in the scale are given in Table IX.

Table IX. *Correlations between time and moves in the performance tests.*

(1) Block-design									
Test	2	3	4	5	6	7	8	9	10
<i>r</i>	0.66	0.80	0.78	0.82	(0.77)	(0.79)	(0.79)	(0.84)	(0.77)
(2) Form-boards					(3) Cubes				
Test	1	2	3		Test	1	2	3	
<i>r</i>	0.91	0.87	0.90		<i>r</i>	0.75	0.72	(0.71)	

Averages: (1) Block-designs:

Average for 4-block tests (2-5), $r=0.77$.

Average for 9-block tests (6-7), $r=0.78$.

Average for 16-block tests (8-10), $r=0.80$.

(2) Form-boards, $r=0.89$.

(3) Cubes, $r=0.73$.

[Coefficients in brackets are for fewer subjects than the whole group; $r=0.25$ is just five times its P.E. for 170 cases.]

Conclusions. The average values in the three groups are a few points higher than those which Kohs obtained for the relation between time and moves in his block-design test(7). His average value is approximately $r=0.7$, but he recorded moves in a slightly different fashion and his instructions possibly did not arouse the same attitude. There would thus be more opportunity for the operation of speed preferences. From his results he concluded that groups may be separated according to their method of work as slow and accurate, rapid and inaccurate, etc. It is difficult to accept this with the relationships between times and moves which he obtained, for these are high, keeping in mind the nature of the variables measured. The existence of such types would seem to be the exception rather than the rule from our data. Here the instructions asked for maximum effort, and this may have made the difference. Under these conditions it seems that the time is entirely governed by the number of steps required. If the latter is determined by the level of intelligence it may be asked here why the correlations between the time in a single problem and the 'level' factor are low. Accidental errors apparently produce the low correlations, for the correlation between

'level' and time rises as more tests are used to get the time measures. For instance, when all nine tests which were used for the speed scores are combined as before, the correlation between this time measure and the total score on the performance test becomes $r=0.73$.

Some practical consequences arise from these results. With performance tests, the times taken to do a few tests of moderate difficulty, such as can be solved by nearly the whole group, give a sufficiently good measure of intelligence to warrant their use in making a rapid survey of a group. Again, when speed preferences are ruled out as far as possible, it is unnecessary to weight both time and moves, as Kohs does, in scoring performance tests.

III. SUMMARY AND CONCLUSIONS.

Previous work shows conclusively that increasing the time allowances in time-limit tests of intelligence does not alter the rankings for intelligence in these tests. This indicates that the rate of work *in these tests* is determined by the level of the intelligence, yet popular opinion is almost unanimous in asserting that a separate ability for speed exists. Probably this is due to the fact that in everyday life people do exhibit a speed factor because they have natural speeds at which they *prefer* to work. In the tests, however, these preferences are overruled by the conditions of working at maximum pressure. Naïve defence mechanisms also preserve the belief in a speed factor.

More important is the acceptance of a factor of speed by psychologists, *e.g.* Thorndike and his workers. Here the factor is conceived as a 'pure speed,' or facility in cognitive processes, and is best manifested where the intellectual difficulty of the material is nearly zero. But such a factor must be eliminated in most of the intelligent reactions required in the tests, since the tasks there have considerable difficulty. Data have been obtained to test the existence of a factor of speed in the solving of tasks such as are used in intelligence tests. The times taken by a group of subjects to solve a number of problems were recorded, and a measure of level of intelligence was also obtained. The times correlated positively, as would be expected, but this relationship practically disappeared when the influence of the 'level' factor was removed. It has therefore been concluded that there is little evidence for an independent factor of speed when the conditions demand a uniform attitude of securing maximum accuracy at the greatest speed. When the problems are of low difficulty a factor of speed comes into operation.

If intelligence is taken as the general factor 'g,' then no specific factor of speed is involved in solving problems, whether these are very easy or more difficult.

By using performance tests it has been found that the times taken for more complicated tasks are highly correlated with the number of steps made. The latter will be chiefly determined by the level of the intelligence, and since the discriminating portion of group tests usually involves harder problems, the absence of a speed factor in these tests can be understood.

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(Manuscript received 6 June, 1933.)

THE RELATION OF INTELLECTUAL, TEMPERAMENTAL AND OTHER QUALITIES TO SUCCESS AT SCHOOL—A FOLLOWING UP ENQUIRY.

BY J. B. RUSSELL.

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I. INTRODUCTION.

WITH the co-operation of local Education Authorities, Prof. Godfrey Thomson's Group test (Northumberland Test No. 1) was used in 1925 in an eastern administrative county and its associated borough for the dual purposes of making a survey of the children of ten and eleven years of age, and a first selection of pupils for admission to secondary schools. A score of 115 I.Q. was fixed as the lower limit for admission to Part II of the examination (English and Arithmetic) with the safeguard that any child put forward by a head teacher or a parent might sit regardless of his score.

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The results of the survey were given in a previous paper (1).

For various reasons, poverty, parental ignorance, geographical difficulties, weakness in Part II of the examination, a number of children who scored 115 i.q. plus (including many 130's) did not enter a secondary school.

The problems considered in this paper.

Although in 1925 free places were awarded solely on the results of Part II of the examination in English and Arithmetic, for which purpose Dr Ballard's standardized Silent Reading and Arithmetical Reasoning Papers (pp. 163 and 193 of *The New Examiner*) were used, the intelligence test score of each successful candidate was known and it appeared that a useful investigation might be carried out by keeping a record for four or five years of the progress of these children through the county secondary schools (cf. (2)).

It is proposed here to consider two problems:

(1) What characteristics are generally associated with a good secondary school record?

(2) How far are secondary school records and the results of the School Certificate examination in agreement with the forecast given at entrance (a) by the group test of intelligence, or (b) by the examination in English and Arithmetic?

The paper may serve as an example of the kind of investigation that the heads of secondary schools, who are interested in these important problems, might carry out with little disturbance of the ordinary routine.

One important point should be specially mentioned. In order that the number of pupils under consideration may be increased, not only the free place candidates, but all the pupils in the secondary school forms which free place pupils enter, should take the tests.

II. PROCEDURE.

The first step was to prepare a record form for use by the staff of the secondary schools. A form of individual record was received from Mrs Susan Isaacs, and ultimately a draft record form for use in this investigation was discussed with Prof. Cyril Burt and Prof. Godfrey H. Thomson. The discussion centred around the 'characteristics' to be recorded and the means of assessing them, but for reasons of policy and of the conditions of work in secondary schools not all the suggestions could be adopted. Moreover, at the end of the period the pupils about whom information was available had been reduced by early leavers and

by transfers elsewhere to the numbers shown in Table I, and these circumstances have added considerably to the difficulty of presenting the facts.

The number of boys and girls under consideration at each secondary school, and other details, are given in Table I.

Table I.

Secondary school	Size of schools	Number of pupils under consideration	Sex
A.B.	300 plus	16	M., F.
B.N.	100-200	12	M., F.
C.S.B.		10	M.
D.S.G.		5	F.
E.H.	Below 100	9	M., F.
Total 52			

Early in 1926 the heads of the secondary schools to which boys and girls had been sent as free place pupils were interviewed and acquainted with the purpose of this investigation, which required the keeping of a full record of the secondary school life of these pupils. Their valuable co-operation was readily obtained. The record forms were collected from five secondary schools after the School Certificate examination in 1930—during the year July 1930-1. At the same time the heads of the schools, in personal interviews with the writer, gave an account of each of the boys and girls who had been with them four or five years. The interviews dealt with outstanding points of character, health and circumstances that had affected the school life of the pupils.

III. DATA ON WHICH THIS PAPER IS BASED.

At the end of the period the information available for each pupil included:

(1) The examination record before and after entering the secondary school, viz. (a) I.Q. scores, 1925; (b) I.Q. order of merit; (c) free place examination marks (English and Arithmetic), 1925; (d) free place order of merit; (e) School Certificate results.

(2) The secondary school record, (a) form position at the end of each term; (b) a 'Final Order' prepared by the heads of the secondary schools based on a consideration of the school record and the School Certificate examination, without regard to other factors.

(3) Marks for 'characteristics' on a seven-point scale (see p. 298): (a) assessed at the end of the first year; (b) the pooled assessment of the staff over a period of four or five years.

(4) An individual record that throws light on points of character, health and home life.

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The tables for each secondary school are somewhat complicated and cannot be published *in extenso*, but the information has been made use of in the discussion of cases where there is a discrepancy between the I.Q. and final secondary school orders.

IV. CHARACTER AND TEMPERAMENT.

For success at school intelligence must not fall below a certain level, different for different individuals, but the brilliant boy with defects of character may fail while the boy of average intelligence coupled with abnormal powers of perseverance and a determination to succeed may surpass comrades possessed of greater inborn potential ability.

The quantitative estimation of traits of character has presented great difficulties and work in this direction has lagged behind the measurement of intelligence, but since the early years of this century definite progress has been made. In *The Abilities of Man* (Chap. xx) Prof. Spearman has reviewed the work down to 1927 including the scientific research of Dr Edward Webb published in 1915 as a monograph by the British Psychological Society (3). Reference should also be made to recent papers by H. G. Stead (4) and David W. Oates (5, 6) on the estimation of temperament and character and to the 1913 report of the Assistant Masters' Association (7).

To secure co-ordination it is the practice to define the qualities of temperament or character to be estimated and to describe the scale of marking. For the purpose of this enquiry the list of qualities included intellectual, temperamental, social, and physical characteristics, together with energy and general emotionality.

Copies of the schedule that follows (without the sub-heads below) were provided for each secondary school together with instructions for marking.

Schedule of characteristics for secondary school record.

Intellectual.

- (1) Quickness of comprehension; alacrity in understanding new material.
- (2) Profoundness of comprehension; completeness with which the new material is grasped. Depth—as distinct from superficial apprehension.
- (3) Soundness of common sense; judgment.
- (4) Originality of ideas; ability to suggest his (her) own solutions of difficult situations.

Temperamental.

- (5) Attention; ability to resist distraction in class and during preparation.
- (6) Persistence; perseverance in spite of difficulty or when the task is disagreeable.

Social.

- (7) Manners. Behaviour—polite or impolite—pleasing or displeasing.
- (8) Leadership; degree to which he (she) spontaneously takes the lead.
- (9) Sociability; preference for being with other people.

Physical.

- (10) Physical fitness; a layman's estimate.
- (11) Sportsmanship; interest in sport and games. Appreciation of the team spirit.

Others.

- (12) Energy; activity in both work and play.
- (13) General emotionality; high excitability of all or most of the emotions as opposed to apathy, stolidity or a placid nature.

Of the intellectual items (1) to (4) are almost identical with the qualities listed by Dr Webb, and (5) and (6) are related to Nos. 33 and 34 of his scheme—No. 33, Tendency not to abandon tasks in the face of obstacles: No. 34, Tendency not to abandon tasks from mere changeability.

Instructions for Marking.

Scale: Plus 3; plus 2; plus 1; 0; -1; -2; -3. Put the plus sign as well as the minus sign. The comparison in every case is with the average boy or girl of the same age known to you.

- (a) The mark plus 3 is for those showing a very high degree of the quality as compared with the average boy or girl of the same age.
- (b) Plus 2. Distinctly above the average.
- (c) Plus 1. Slightly above the average.
- (d) 0. Possessing the average degree of the quality.
- (e) Minus 1. Slightly below the average.
- (f) Minus 2. Distinctly below the average.
- (g) Minus 3. Showing the lowest degree.

Independent estimates each July by the form master or mistress were asked for, but only one record form was used for each pupil. In this connection Prof. C. W. Valentine writes: "It is, I think, better to have an assessment made by three teachers in collaboration than to blend assessments made independently. In judging 'Trustworthiness,' for example, one glaring default, known only to teacher A, may probably influence the mark of the other two, B and C, if told to them, whereas the low mark of teacher A might otherwise be swamped by the good marks from B and C." (8). Burt has suggested means whereby the ratings of different observers may be made more nearly comparable and the ratings of the same observer kept more closely consistent amongst themselves; but in this case the secondary schools were widely distributed, and it was not possible for the observers to enjoy a first-hand intimacy with representative children picked out at intervals along a percentile scale to serve as common and constant points of reference (9).

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The sequel will indicate what results it is possible to obtain in relatively small secondary schools without elaboration of technique.

Table II. *School B.N.*

Group	Pupils	Final order	Quickness of comprehension	Profoundness of comprehension	Soundness of common sense	Originality of ideas	Attention	Persistence
(a)	E.	1	2.0	2.0	2.0	2.0	0.2	2.0
	A.	2	1.0	1.6	1.0	1.0	0.8	1.6
	C.	3	1.0	1.0	0.8	0	1.6	1.0
(b)	D.	4	1.4	2.8	1.8	1.6	2.0	2.0
	L.	5	0.75	1.75	1.75	1.5	-0.25	0.5
	G.	6	1.0	0	0	0	0	0
	B.	7	-0.6	-0.6	0	-0.6	0	0
	I.	8	0.4	0	0.2	0.2	0	1.0
	J.	9	-1.0	-0.6	0	-1.0	0	0
(c)	K.	10	-0.2	-0.6	-0.2	0	-0.6	0
	H.	11	-1.2	-1.2	0	-1.0	0	0
	F.	12	-0.4	0	0	0	-0.4	-0.6

Average scores of groups (a), (b) and (c).

Group	Quickness of comprehension	Profoundness of comprehension	Soundness of common sense	Originality of ideas	Attention	Persistence
(a)	1.33	1.53	1.26	1.0	0.86	1.53
(b)	0.32	0.56	0.62	0.28	0.29	0.58
(c)	-0.6	-0.6	-0.6	-0.33	-0.33	-0.20

Scale differences between groups (a) and (b), and (a) and (c).

Groups	Quickness of comprehension	Profoundness of comprehension	Soundness of common sense	Originality of ideas	Attention	Persistence
(a) and (b)	1.01	0.97	0.64	0.72	0.57	0.95
(a) and (c)	1.93	1.80	1.32	1.33	1.19	1.73

School A.B.

For the groups (a), (b) and (c) in a second school A.B., with sixteen pupils, the results are given below.

Average scores of groups (a), (b) and (c): extremes in brackets.

Group	Quickness of comprehension	Profoundness of comprehension	Soundness of common sense	Originality of ideas	Attention	Persistence
(a)	1.25	1.55	1.2	0.7	1.75	1.4
	(2.2, 0)	(2.2, 1)	(1.4, 1)	(1.2, 2)	(2, 1)	(2.2, 0.6)
(b)	0.12	0.37	0.2	0	0.9	0.84
	(0.6, -0.25)	(0.8, 0)	(1, 0)	(0.25, 0)	(2, 0)	(2, 0.2)
(c)	-0.6	0.1	-0.1	-0.2	0.11	0.34
	(0.8, -1)	(0.4, 0)	(0.8, -0.5)	(0.8, -1)	(0.25, 0)	(1, -0.25)

Scale differences between groups (a) and (b), and (a) and (c).

Groups	Quickness of comprehension	Profoundness of comprehension	Soundness of common sense	Originality of ideas	Attention	Persistence
(a) and (b)	1.13	1.18	1.0	0.7	0.85	0.56
(a) and (c)	1.85	1.45	1.3	0.9	1.64	1.06

V. INTELLECTUAL AND TEMPERAMENTAL QUALITIES.

The first problem is to find out which of the qualities under consideration are associated with good secondary school records. We will first consider the qualities labelled 'Intellectual' and 'Temperamental' in relation to orders of merit (named 'final orders' in the text) prepared by the heads of secondary schools, after consideration of form orders, and the result of the School Certificate examination. For convenience of study the final orders in the larger schools B.N. and A.B. have been divided into three parts:

A top section (a)—above the 1st quartile.

A middle section (b)—the interquartile range.

A bottom section (c)—below the 3rd quartile.

The pooled scores of a number of judges (three to five), extending as a rule over four or five years, have been entered in Table II for the larger schools B.N. and A.B. so that the differences between the average scores of the groups (a) and (b), and (a) and (c), may be observed with a view to determining how pupils with the highest secondary school record compare with the middle and lowest sections.

Taking into consideration the tabulated qualities, it is clear that the scores for the groups (a), (b) and (c) are in descending order, and that the (a) scores greatly exceed (c). Thus an inspection of the detailed tables coupled with the evidence of coefficients of correlation gives a general picture of the group, and helps in the solution of the problem under consideration.

Coefficients of correlation (r) have been calculated for the Final Orders of two secondary schools and each of the qualities 1 to 6, and for the combinations 1 to 4, 1 to 6 and 5 to 6 inclusive (see Table III).

Table III. *Correlations of final secondary school orders and (1) Quickness of comprehension; (2) Profoundness of comprehension; (3) Soundness of common sense; (4) Originality of ideas; (5) Attention; (6) Persistence.*

School	No. of pupils	Quality									P.H.	
		1	2	3	4	5	6	1-4	1-6	5-6	Min.	Max.
A.B.	16	0.63	0.88	0.84	0.63	0.86	0.63	0.72	0.88	0.93	0.023	0.102
B.N.	12	0.81	0.76	0.76	0.63	0.72	0.84	0.81	0.76	0.81	0.057	0.102
Average		0.72	0.82	0.80	0.63	0.79	0.73	—	—	—	—	—

The reliability of the marking by secondary schools is indicated below, where, omitting 'Attention,' the order of scale differences expressed

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in marks for groups (a) and (c), beginning with the highest difference, is, for A.B. and B.N., similar (see Table IV).

Table IV.

	School A.B.		School B.N.	
	Differences between groups (a) and (c)	Order	Differences between groups (a) and (c)	Order
Quickness	1.85	(1)	1.93	(1)
Profoundness	1.45	(2)	1.80	(2)
Common sense	1.30	(3)	1.32	(4)
Persistence	1.06	(4)	1.73	(3)
Originality	0.90	(5)	1.33	(4)

VI. PHYSICAL, SOCIAL AND EMOTIONAL QUALITIES.

It is now necessary to consider the social, physical and other characteristics of the secondary school record and their relationship (if any) to the final orders. For this purpose the average scores of the sections (a), (b) and (c), and the scale differences (a) and (b), and (a) and (c), are given below (see Tables V and VI).

Table V. *School B.N.*

Groups	Average scores of groups (a), (b) and (c).						General emotion-ality
	Social			Physical		Energy	
	Manners	Leader-ship	Socia-bility	Physical fitness	Sports-manship		
(a)	0.66	0.06.	0.06	0.66	0.80	0.40	0.66
(b)	0.33	-0.30	0.39	0.18	0.03	0.42	0.50
(c)	0	-0.73	-0.02	-0.06	0.13	-0.40	-0.40
Scale differences between groups (a) and (b), and (a) and (c).							
(a) and (b)	0.33	0.36	0.33	0.48	0.77	0.02	0.16
(a) and (c)	0.66	0.79	0.08	0.72	0.67	0.80	1.06

Table VI. *School A.B.*

Groups	Average scores of groups (a), (b) and (c).						General emotion-ality
	Social			Physical		Energy	
	Manners	Leader-ship	Socia-bility	Physical fitness	Sports-manship		
(a)	1.10	-0.10	0.45	0.45	0.75	0.60	-0.05
(b)	0.54	-0.68	-0.22	-0.27	-0.09	0.06	0
(c)	0.72	-0.85	0.30	-0.60	-0.31	-0.10	0.25
Scale differences between groups (a) and (b), and (a) and (c).							
(a) and (b)	0.56	0.58	0.67	0.72	0.84	0.54	0.05
(a) and (c)	0.38	0.75	0.15	1.05	1.06	0.70	0.30

Except in the case of physical qualities the scale differences are much lower than in the previous tables.

Manners. Scale differences are small, and there is no gradation of marks from (a) to (c) except in B.N.

Sociability. Again there is irregularity except in schools D.S.G. and E.H., where the lowest pupils in the final order have the highest marks.

Leadership. The marks for (a), (b) and (c) are in descending order but the differences are small. In A.B., three of the four in group (a) score between 0 and 0.5, and in (c) a like number score between 0 and -2.

It is noteworthy that leadership is not a characteristic of this selected group for, of the fifty-two pupils under consideration, 48 per cent. have been awarded a minus mark. In A.B. the (a), (b) and (c) groups, and in B.N. the (b) and (c) groups are awarded minus marks. Moreover, none of the seven (a) children in A.B. and B.N. has a plus 1 mark.

Physical fitness. For the larger schools A.B. and B.N. there is a regular gradation from (a) to (c), and the scale differences are important. Four-sevenths of the (a) groups score plus marks, and the same proportion of the (c) groups are marked minus. The correlations with the final orders are positive

$$\left. \begin{array}{l} r \text{ for A.B.} = 0.43 \\ r \text{ for B.N.} = 0.63 \end{array} \right\} \text{Average } 0.53.$$

While 50 per cent. of the (a) groups score a plus mark and 16 per cent. a minus mark, the corresponding figures for the fifty-two pupils are—plus, 36 per cent.; and minus, 27 per cent.

Energy. The scale differences are in descending order except in school D.S.G., but the differences are not marked, and the range is usually plus 1 to minus 1.

General emotionality. The differences are not very helpful but individual marks are significant.

The following is the distribution:

	Plus	0	Minus
For the whole group—52 pupils	17	19	16
For the top 12 in the final orders (a)	5	5	2
For the lowest 12 in the final orders (c)	5	2	5

Thus the (a) group is distinguished by normality or plus marking, and the (c) group by somewhat greater deviations from the normal. The majority come within the range plus 1 to minus 1.

VII. COMPARISON OF SECONDARY SCHOOL FINAL ORDER WITH THE ORDER IN THE NORTHUMBERLAND TEST NO. 1.

It will now be of interest to consider how far the secondary school final orders agree with the order of merit in the intelligence test, and to examine notable differences¹.

¹ For school A.B. the value of r is 0.56 ± 0.115 and for school B.N., 0.54 ± 0.137 . It is to be noted that the correlations are based on small numbers because it was not possible to amalgamate the secondary school orders. Actual differences in rank are given in Tables VII and VIII. The correlations would have been higher had all pupils who scored high I.Q.'s been admitted (see Introduction, p. 296).

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The arrangement of the data in Tables VII and VIII indicates the nature and distribution of the rank differences. In Section 1 of Table VII, the differences in rank are small and the pupils are grouped under three heads:

- (a) Those with a high position in test and final order.
- (b) A middle group.
- (c) Those with a low position in test and final order.

Table VII. *School A.B.*

Section 1. Rank differences not more than four places out of sixteen.

Pupils	(a) High in test and final order				(b) Middle group				(c) Low in test and final order		
	A.	B.	C.	D.	E.	F.	H.	I.	K.	L.	M.
Rank in I.Q.	1½	1½	3	4	5	7	7	9	11	12	13½
Rank in final	5	1	2	4	8	10	3	11	12	13	16
Differences in rank	3½	½	1	0	3	3	4	2	1	1	2½

Section 2. Rank differences of more than four places out of sixteen.

Pupils	Higher in I.Q. Lower in final		Higher in final Lower in I.Q.		P.
	G.	J.	N.	O.	
Rank in I.Q.	7	10	13½	15½	15½
Rank in final	14	15	6	9	7
Differences in rank	7	5	7½	6½	8½

Looking at the figures in Section 1, there is a close agreement between the Final Order and the I.Q. results obtained in 1925. The rank differences do not exceed 25 per cent. of the number of pupils in the secondary school group and the deviations are often much less than this. A. and H. alone call for comment. The drop from 1½ to 5 in case A. is amply accounted for by certain unhappy circumstances recorded in the individual report of the headmaster. As to pupil H. the pooled record of his characteristics by observers over a period of five years is perhaps sufficient to account for his higher rank in the Final. His marks, all plus, were: Attention 2; Quickness of comprehension, 0; Profoundness of comprehension, 1; Persistence, 1; Common sense, 1; Energy, 1·2; Physical fitness, 1·8; General emotionality, 0.

In Section 2 of Table VII the differences in rank are wider, and the grouping differentiates between those who took a higher position in the final than in the test and *vice versa*. On consideration of these discrepant cases the following extracts from individual reports and available data

(*E*, elementary school, 1925, and *S*, secondary school, July 1930-1) appear to be relevant:

- G. I.Q. order, 7/16 (7th out of 16). Secondary school final order, 14/16.
 Age on entry, 11.5. I.Q. 133. (Range 153 to 115.)
 Entrance examination in Arithmetic and English, 9/16.
 Form position at end of 1st year, II *a*, 30/35.
 Form position at end of 4th year, IV *a*, 19/31.
 (*E*) Interest—books.
 (*S*) Memory and reasoning power below the average. Mathematics weak and little interest in books. Pleasant, sociable and in conversation of average ability, but in no sense of the word a student.
 The pooled assessment of characteristics by four judges over a period of five years are: Persistence, 0.6; Physical fitness, -0.6; for the rest, 0.
- J. I.Q. order, 10/16. Secondary school final order, 15/16.
 Age on entry, 10.10. I.Q. 129.
 Arithmetic and English order, 6½/16.
 Form position at end of 1st year, II *b*, 14/34.
 Form position at end of 4th year, IV *b*, 20/28.
 (*E*) Interest—Persons.
 (*S*) Poor health and in consequence irregular attendance. Academic career unsuitable.
 Ten of thirteen qualities are assessed with a minus sign. Physical fitness, General emotionality, Quickness of comprehension, Originality of ideas: all -1.
- N. I.Q. 123. O., P. I.Q. 115. Although low in the I.Q. order, each of these pupils had sufficient ability to profit by a secondary school career. In two of the three cases persistent hard work enabled them to forge ahead of abler comrades.
- N. I.Q. order, 13½/16. Secondary school final order, 6/16.
 Age on entry, 12.3. I.Q. 123.
 Arithmetic and English order, 16/16.
 Form position at end of 1st year, II *a*, 5/35.
 Form position at end of 4th year, V *b*, 1/23.
 (*E*) Stolid. Persevering. Interests—books and gardening.
 (*S*) Success due to persistent hard work, rather than to superior intelligence. Attention, 2; Persistence, 2; Energy, 1; General emotionality, 0.
 Passed School Certificate with credits in Mathematics, Geography and Art.
- O. I.Q. order, 15½/16. Secondary school final order, 9/16.
 Age on entry, 12.2. I.Q. 115.
 Arithmetic and English order, 15/16.
 Form position at end of 1st year, II *b*, 5/34.
 Form position at end of 4th year, V *b*, 9/20.
 (*E*) Quiet and reserved. Interest—books.
 (*S*) Attention, 0.8; Persistence, 0.8. Two qualities only marked minus.
 Passed School Certificate with two credits.

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P. I.Q. order, 15½/16. Secondary school final order, 7/16.

Age on entry, 12.4. I.Q. 115.

Arithmetic and English order, 11/16.

Form position at end of 1st year, II b, 9/35.

Form position at end of 4th year, V b, 2/23.

(E) Of retiring nature, with little interest in games.

(S) Not always bright but a hard worker. Attention, 1.2.

Passed School Certificate in 1930 with five credits.

N. and P. came from small rural elementary schools, which on a five-point scale A. to E. would be marked B.

The explanations given here appear to be sufficiently significant to account in large measure for four of the five cases showing a wide divergence between the I.Q. record and the final order:

To contrast with the discrepant cases, some details are now given of pupils whose rank order in the final was very near the rank order in the intelligence test.

D. I.Q. order, 4/16. Final order, 4/16.

Age on entry, 11.2. I.Q. 144.

Arithmetic and English order, 3½/16.

Form position at end of 1st year, II b, 9/34.

Form position at end of 4th year, V b, 4/23.

(E) Industrious and eager to find out things alone. Interests—books and things.

(S) Very intelligent and also a hard worker. Did not develop very quickly but made excellent progress during the past two years. Good all round, and best in Mathematics and Science. Good athlete.

Marks for characteristics, all plus, including: Quickness, 1.6; Profoundness, 1.0;

Common sense, 1.2; Leadership, 0.4.

Passed in 1929, and obtained Honours and seven credits in 1930.

C. I.Q. order, 3/16. Final order, 2/16.

Age on entry, 10.11. I.Q. 150.

Arithmetic and English order, 1/16.

Form position at end of 1st year, II a, 2/35.

Form position at end of 4th year, V a, 3/26.

(E) Absent from school for periods through ill-health which undoubtedly affected character and temperament. Reserved, few friends. Interest—books.

(S) Intelligent and a very hard worker. A student more than anything else.

Reserved and apt to be unsociable.

Quickness; Profoundness; Attention; Persistence: all 2 plus. Leadership, 0.2.

Passed with Honours and six credits in 1929.

K. I.Q. order, 11/16. Final order, 12/16.

Age on entry, 11.11. I.Q. 127.

Arithmetic and English order, 14/16.

Form position at end of 1st year, II b, 13/34.

Form position at end of 4th year, IV a, 20/31.

(E) Interests—books and persons.

(S) Up to 1925 health indifferent and in consequence this pupil had little ground-work. Not unintelligent and failure due to health trouble in early years.

Attention and other characteristics mainly 0. Leadership, -1.0.

A classification of school B.N. on the lines of that for school A.B. is given below.

Table VIII. *School B.N.*

Section 1. Rank differences not more than three places out of twelve.

Pupils	(a) High in test and final order			(b) Middle group		(c) Low in test and final order		
	A.	C.	D.	G.	I.	H.	J.	K.
Rank in i.q.	1	3	4	$6\frac{1}{2}$	9	8	$10\frac{1}{2}$	$10\frac{1}{2}$
Rank in final	2	3	4	6	8	11	9	10
Differences in rank	1	0	0	$\frac{1}{2}$	1	3	$1\frac{1}{2}$	$\frac{1}{2}$

Section 2. Rank differences of more than three places out of twelve.

Pupils	Higher in i.q. Lower in final		Higher in final Lower in i.q.	
	B.	F.	E.	L.
Rank in i.q.	2	$6\frac{1}{2}$	5	12
Rank in final	7	12	1	5
Differences in rank	5	$5\frac{1}{2}$	4	7

The following are observations on the discrepant cases:

B. i.q. order, 2/12. Secondary school final order, 7/12.

Age on entry, 11.7. i.q. 140.

Arithmetic and English order, 5/12.

Form position at end of 1st year, II, 12/33.

Form position at end of 4th year, IV b, 6/26.

(E) Less intelligence than the i.q. would lead one to expect. Good powers of concentration.

(S) Careful, diligent, slow.

For characteristics there is no score above 0.

Did not take School Certificate.

F. i.q. order, $6\frac{1}{2}$ /12. Secondary school final order, 12/12.

Age on entry, 11.10. i.q. 128.

Arithmetic and English order, 10/12.

Form position at end of 1st year, II, 9/33.

Form position at end of 4th year, absent.

For characteristics there are eight minus scores, and among them Attention, -0.4; Persistence, -0.6; Energy, -0.6; Physical fitness, -0.2; General emotionality, -0.2.

The pooled marks are lower than those for the 1st year.

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- E. i.q. order, 5/12. Secondary school final order, 1/12.
 Age on entry, 11.2. i.q. 131.
 Arithmetic and English order, 1½/12.
 Form position at end of 1st year, II, 3/33.
 Form position at end of 4th year, V, 1/13.
 (E) Interest—books and things. Persevering.
 (S) For characteristics (1-6) the second best record of the group. For all intellectual qualities, plus 2. Persistence, plus 2. Physical fitness, plus 1.
- L. i.q. order, 12/12. Secondary school final order, 5/12.
 Age on entry, 12.10. i.q. 106.
 Arithmetic and English order, 3/12.
 Form position at end of 1st year, II, 2/33.
 Form position at end of 4th year, V, 5/13.
 (E) Interests—books and things.
 (S) For Profoundness of comprehension, Common sense, Originality, 1.5 plus; Physical fitness, 0.5; Energy, 0.75; General emotionality, 2.0; Attention, —0.25. Passed School Certificate in 1929 with a 'good' in Mathematics and a distinction in Art¹.

VIII. COMPARISON OF SECONDARY SCHOOL FINAL ORDER WITH 1925 ENTRANCE EXAMINATION ORDER IN ARITHMETIC AND ENGLISH².

Expressed in terms of the sum of differences in rank the figures are:

	A.B., B.N.	Five secondary schools
(a) Sum of differences in rank between secondary school final orders and i.q. orders	85	139
(b) Sum of differences in rank between secondary school final orders and English and Arithmetic orders	100	181

It is probable that the wider differences in the five secondary schools between (a) and (b) may be accounted for by the method adopted in making an age allowance in the entrance examination in English and Arithmetic, for it has been found by an independent examination of two groups of 41 and 42 pupils that there is a close correspondence ($r=0.8$ plus) between the results of the Northumberland Test No. 1 and the results of an examination in Dr Ballard's English and Arithmetic papers

¹ H. G. Stead(10), working with larger numbers, has made a more comprehensive classification of pupils in his paper, "Factors in Mental and Scholastic Ability," and C. W. Valentine in his published volume gives an interesting account of an enquiry into the reliability of examinations with special reference to entrance examinations to secondary schools, the School Certificate examination, and the award of scholarships at Universities(11).

² For these orders the average correlation (r) for the groups in the larger schools A.B. and B.N. is 0.40 (r.e. ± 0.116).

when a correction for age is made by the calculation of educational quotients¹.

	No. of pupils	Age range	Range of I.Q.	Range of B.Q.	<i>r</i>	P.B.
(a) Town A	41	10.3-12.1	85-142	70-155	0.81	± 0.038
(b) Town B	42	10.2-12.1	85-147	55-144	0.80	± 0.038

IX. RELATION BETWEEN THE RESULT OF THE NORTHUMBERLAND TEST
No. 1 (1925) AND THE RESULT OF THE SCHOOL CERTIFICATE
EXAMINATIONS OF 1929 AND 1930.

Unhappily it is not possible to present the whole of the table of results, but a useful summary of the facts is obtained by considering (a) the 'credits' in relation to intelligence quotients, (b) the failures, (c) pupils who did not sit, and, lastly, (d) the discrepant cases.

(a) Comparison of I.Q. groups with average number of credits.

No. of pupils	I.Q. groups	Average number of credits
3	150 plus	6.0
5	140-149	4.8
15	130-139	4.4
16	120-129	1.4
9	110-119	1.0
4	100-109	3.5
Total	52	

Thus, on the whole, neglecting for the moment the lowest group, those pupils with high mental ratios acquitted themselves better than those with lower mental ratios, but all the boys and girls with high I.Q.'s (130 plus) did not do well—three failed; one in French only, and two for reasons other than want of intelligence. The correlation (*r*) between the respective orders of merit in the two examinations is 0.53 ± 0.065 .

(b) Of the six who scored 110 plus and failed:

- Two were ill—one definitely so, and the other was physically weak.
- One had cause for great emotional disturbance.
- One was not a student and was marked minus for physical fitness.
- One was reported as 'slack.'
- One failed in French only.

(c) Of the twelve who did not sit for the School Certificate:

Five were marked minus for physical fitness and the following are relevant observations made by their teachers:

¹ An important contribution to the vexed question of age allowance has been made by Prof. Godfrey H. Thomson in his papers on the "Standardization of group tests and the scatter of intelligence quotients" (12).

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Two were said to be 'slow'; both were interested in 'things,' rather than in books.

One had ability, but no interest in books.

One had no ambition—interest in 'things.'

Three left in 1929 before taking the examination.

Two had unfavourable home conditions.

One was not a worker.

Seven of the twelve were assigned minus marks in the pooled estimates for three of the intellectual qualities—quickness and profoundness of comprehension, soundness of common sense, and originality of ideas. Two of the seven also received minus marks for 'attention' or 'persistence.'

For the first four in group (c) a normal secondary school course was probably unsuitable.

(d) It is somewhat surprising however that three of the four pupils R., S., T., who scored only average i.q. marks (100–109) did so well in the Certificate examination. In this connection Prof. Thomson writes, "Persistence and hard work may explain many a slowly developing child without any need to suppose that he had become more intelligent; and lack of those qualities may explain the reverse phenomena without any actual change in intellect having occurred" (13).

Pupil R., for example, scored 107 i.q. and obtained only 67 marks for Arithmetic and English and yet he did as well as Pupil X. whose i.q. worked out at 144 and who scored 129 marks for Arithmetic and English. In his first year at the secondary school R. was marked plus 2 for Quickness and Profoundness of comprehension, Attention, Persistence and energy, and in the last year his marks for the same qualities were raised to plus 3. Similarly Pupil S. was marked plus 1 for Quickness of comprehension, Attention and Persistence in the first year, plus 2 for Persistence in the second, third and fourth years, and plus 2 for the qualities (1) to (6), except 'Originality' in the third and fourth years. Plus 2 for Profoundness of comprehension was the highest mark scored by T. Plus 1 for Persistence in the fourth year is also notable. On considering the annual marks for qualities (1) to (6) (p. 298), energy and physical fitness, awarded to pupils R. and S., there appears to be evidence of growth in intellectual and temperamental qualities, but on examination of the marks for other pupils in this school a marked increase in the estimates from year to year is revealed. Thus R. and S. are not peculiar in this respect and the hypothesis of growth breaks down. Further enquiry into the case of R. throws some light on the discrepant result. During the period 5–7 this pupil was delicate and he was not promoted from an infants' department to the elementary school until 7 years 11 months. Thus he spent nearly a year less than the normal period in

the elementary school before taking the free place examination. This alone is probably the explanation of comparatively low marks in English and Arithmetic at 11 years of age. This pupil's strongest subject was Mathematics and his weakest English. Any weakness in ability to read or to understand English at the date of the test would certainly be revealed in the Northumberland Test score, for explanations of the sub-tests need first to be read and understood. Further evidence is available. Home conditions and parental influence resulted in consistent good home work; over a period of years, and were factors of importance leading to success in the Certificate examination. It may be added that the experimental group of 52 pupils when compared with official figures (1930) for the country as a whole differs little from the normal.

	Sat %	Passed %	Failed %
Country—Boys	67.7	55.4	12.3
Girls	63.0	52.3	10.7
Experimental group	67.3	55.8	11.5

X. SUMMARY.

1. (a) Attention and Persistence as defined in the schedule of characteristics are of fundamental importance.

(b) The qualities (1) and (2), Quickness and profoundness of comprehension, (3) Soundness of common sense, and (4) Originality of ideas, are associated with school success. (4) appears to be of less prognostic value than (1) to (3).

(c) A predominant interest in 'books' as distinct from an interest in 'persons' or 'things,' at age 10-12, is a characteristic of many pupils who succeed on transfer to secondary schools.

(d) Social qualities and Leadership bear little relationship to school success.

(e) Of other qualities 'Physical fitness' is of nearly the same importance as 'Intelligence.'

2. (a) When the rank orders in the Northumberland Test No. 1, and in the secondary school Final Orders are compared, there is substantial agreement in 61 per cent. of the cases under consideration.

(b) 'Wider differences' between the Final Order and the test are often due to temperamental or health factors. In a few instances, reasons for the deviations have not been discovered.

(c) The average number of 'credits' gained by candidates at the School Certificate examination is, as a rule, consistent with the mental

ratios determined four or five years earlier. Not all candidates with high mental ratios succeed and not all candidates with average intelligence fail.

I desire to acknowledge the services of the many teachers whose co-operation has made possible the preparation of this paper, and to express my thanks to Prof. Godfrey H. Thomson, Prof. C. W. Valentine and Mr P. B. Coles for criticisms and helpful comments.

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(Manuscript received 1 June, 1933.)

AN INVESTIGATION OF THE RELATION BETWEEN DISCOMFORT AND DISABILITY RESULTING FROM GLARING LIGHT.

By M. R. HARBINSON AND F. C. BARTLETT.

(From the Cambridge Psychological Laboratory.)

- I. *The problem* (p. 313).
- II. *The experiments* (pp. 314-15).
- III. *Experimental results. Series I* (p. 315).
- IV. *Experimental results. Series II* (pp. 315-16).
- V. *Notes on individual cases* (pp. 316-17).
- VI. *Discussion* (pp. 317-18).
- VII. *Summary* (pp. 318-19).
- References* (p. 319).

I. THE PROBLEM.

SEVERAL experimenters have distinguished between 'discomfort' and 'disability' glare (1), and some have suggested that the underlying physiological processes are different. The most definite statements have been made by W. S. Stiles (2), who says: "Reduction in ability to see is an effect independent of discomfort in that, although it often accompanies discomfort, this does not always occur, and, on the other hand, the ability to see may be reduced without perceptible discomfort."

Now discomfort is obviously an experience, and if it is to be measured and accounted for at all, it requires psychological methods of investigation. The questions involved may be treated from at least two different points of view. In the first place 'discomfort' may be regarded as primarily a function of the stimulus. A glare stimulus, for example, if it produces any noticeable effects at all, may, up to a certain degree of intensity, produce discomfort, and beyond that intensity may produce disability. On the other hand, the distinction may be more a function of the task than of the stimulus. Then an objective situation may seem to produce now discomfort and again disability, though so far as the glare itself goes the two situations may be exactly alike. We resolved first to investigate the latter possibility.

II. THE EXPERIMENTS.

We tried a number of preliminary experiments and eventually arrived at the following method:

The observer sat facing a black cardboard screen, 1 ft. high and $4\frac{1}{2}$ ft. long: no head movements could be made. Immediately in front of him was a fixation point; to the right and left two small apertures appeared in the screen, separated by about 90 cm. On an endless band a series of simple dot patterns was arranged. At a pre-arranged signal one of these patterns was exposed at one of the apertures, and as it disappeared another pattern appeared at the other aperture. The alternation of the patterns was continuous throughout a period of work. The observer's task was to call out the number of dots in each pattern, and to do this he had to move his eyes rapidly from one aperture to the other throughout the experimental period. In each pattern the number of dots was near the limit of the span of apprehension, so that the number could be appreciated at a single glance. The speed of exposure could be controlled by the experimenter, and was such that rarely or never could the observer actually count the number of dots. The task therefore demanded considerable concentration, could never be completely mechanised, and any increase in the speed of exposure greatly increased the difficulty of the work. Speeds used ranged from those demanding 40 to those demanding 150 horizontal eye movements each minute.

We chose definitely experiments involving eye movement because Dodge has demonstrated (see *e.g.* (3), pp. 30 ff.) that they are a particularly favourable case for the observance of certain fatigue phenomena. We thought it possible that should the discomfort or disability produced by glare be due in any marked measure to visual fatigue, our technique would throw this fact into prominence. We shall not, however, deal with this specific question in the present paper.

Two general sets of conditions were used for the experiment:

- (1) A good standard of illumination was provided without glare.
- (2) A glaring source of light was introduced directly above the centre of the screen, giving an illumination of 154 ft. candles at the plane of the eye. (No complication was produced by the appearance of visual after sensations, for the observer was all the time moving his eyes rapidly from one aperture to the other, and in no instance was he looking directly into the glare source.)

All observers worked for periods of $1\frac{1}{2}$ min., this period having been found to be within the limit of reasonable efficiency under normal con-

ditions for the task set. Results were scored according to the number of patterns missed. Considerable practice was necessary in each case, in order to eliminate the effects of learning and of general adaptation to the conditions of the experiment. Very great individual differences were found in the speed at which observers could call out all the numbers correctly. Such differences remained in spite of practice.

Ten observers in all took part in the experiment, and their results all showed the same trend. Four of these were examined repeatedly, and it is from their readings that the detailed results given below are taken.

III. EXPERIMENTAL RESULTS. SERIES I.

In the first series of experiments the speed at which each observer, when practiced, could call out the numbers of dots successively exposed with comparatively few mistakes was experimentally ascertained. Patterns were then exposed at the same speed in the presence of the glare light. Each observer gave several sittings, and the order in which normal and glare conditions were exposed was interchanged.

Every observer described the glare light as extremely unpleasant, and continued to do so throughout the course of the experiment. In one case only, however, that of Observer 2 in Series II, was there consistent work decrement with glare. Observation of the subjects indicated that they were all making additional efforts whenever the glare was present, gripping the sides of the chair in which they sat, moving restlessly, getting breathless and flushed or pale and tense, but still no decrement in the amount or quality of the work which they performed was observable.

IV. EXPERIMENTAL RESULTS. SERIES II.

We now proceeded to make the task more difficult, keeping the other objective conditions constant. The speed of exposure, without and with glare, was increased, and the patterns of dots made somewhat smaller. Again each of our four principal subjects attended the experiment many times over, and the number of missed patterns for a given condition remained fairly constant from day to day. Disability now replaced, or accompanied, discomfort as a result of the glare. The curves shown in Fig. 1 represent the average work decrement shown by four subjects under glare conditions at different speeds of exposure of the patterns. For the purpose of comparison the glare effect index is taken as the ratio of misses with glare to misses without glare.

An exactly similar trend of results was shown by all observers tested.

It appears that for this type of task, and under these conditions of observation, there is for all subjects a speed of exposure, or alternatively a difficulty of work, at which the relative disability produced by glare is at

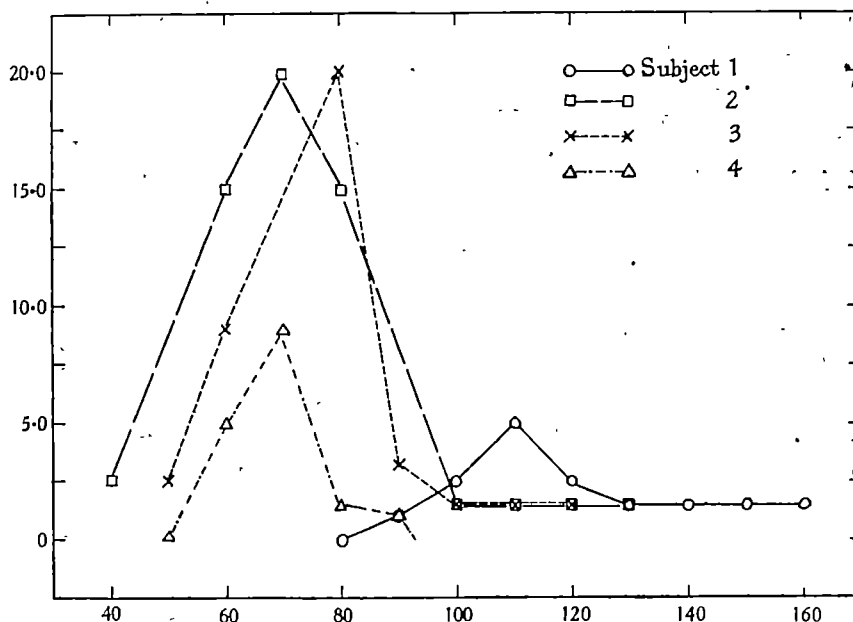


Fig. 1. Curve showing relation between glare decrement and difficulty of task.

Ordinates: $\frac{\text{No. of errors with glare}}{\text{No. of errors without glare}}$. Abscissae: No. of patterns presented per min.

a maximum. With easier tasks glare produces no decrement, and with more difficult tasks the performance under glare is not very markedly different from that under normal lighting conditions.

V. NOTES ON THE INDIVIDUAL CASES.

Observer I. This subject was a man highly practiced in experimental observation. He proved able to work satisfactorily at considerably higher speeds under normal conditions than any other subject examined. Consequently glare disability occurs in his case only at a relatively high speed of exposure.

Observer IV. This subject had poor visuo-muscular control, and at all speeds of exposure above about 90 a minute he missed at least half the patterns presented. Nevertheless he, like the others, showed a zone of difficulty at which the glare stimulus had a maximal effect.

Observers III and II. Both of these observers were very sensitive to the effects of glare, so that for Observer II in particular there was no speed of exposure under glare which did not give rise to some disability. Nevertheless both showed a maximum glare effect with a certain difficulty of task, and this effect was remarkably consistent for a period of several weeks.

VI. DISCUSSION.

It appears to follow from the experiments just described that "glare discomfort" is, at least in an important part, a function of the task rather than of the stimulus directly. If the task is such that it can be fairly readily performed under normal conditions, when glare is introduced the observer makes an extra effort and carries on successfully. The only easily observable effects are to be found in his introspections and in his overt behaviour. But when the task is such that a maximal amount of effort is required under normal conditions, the effect of the glare is to produce an actual deterioration of results. The observer may, however, be so concentrated on what he is doing, judging that the work set *can* be done in spite of the abnormal conditions, that he forgets or does not notice any discomfort and fails to report it, or even in some cases to display it in his behaviour. When the work is so difficult that even under normal conditions it fails to be done efficiently, the introduction of glare may produce further decrement, but the relative deterioration will be less. At this stage, however, discomfort may once more appear prominently in the introspections, for the observer, tending to regard the work demanded as in any case impossible, ceases to be interested in the task.

In discussing the probable physiological mechanism of glare discomfort, Stiles says (2), pp. 308-9): "The sensation is in great part localized at the glare-source of the image, and the brightness, as well as the total light in the image may both be controlling factors in the effect. Shift of the glare source on the retina will change the localized discomfort effect only to the extent that the intrinsic properties of the retina change from place to place. Variation of retinal sensitivities does, of course, occur, but such changes are comparatively gradual."

Our own experiments suggest that much of what passes as discomfort is a central rather than a peripheral phenomenon. Localization of discomfort, in this particular instance, at the "glare-source of the image" is an observation extremely difficult to make with certainty. Naturally the source of the discomfort is commonly referred to the abnormal

stimulus, but that is localization of a totally different order. Peripheral factors of course play their parts. When an unusual stimulus is introduced a completely new set of afferent processes have somehow to be built into those which normally accompany the performance of any task involved. The performer is then called upon to mobilize new resources, and either to make extra efforts, or else to change his method of work in order to effect the desired integration of the novel stimuli with the old. If the extra effort or the change of method is successful, the actual results achieved suffer no deterioration, and all that is left to be observed is a change of behaviour, while all that is left for subjective report is an assertion of discomfort. The discomfort certainly arises from the new stimulation, but it is not itself directly a peripheral phenomenon. Moreover, in many instances, discomfort is a function of the difficulty of a given operation, since, as we have shown, exactly the same objective conditions may give rise, at one stage to discomfort without disability, and at another stage to disability with or without discomfort.

In general then discomfort is a sign of incomplete or faulty adaptation. We would tentatively suggest, holding the view subject to further experimentation, that if discomfort persists as a subjective experience for long periods of trial probably peripheral factors combine with the others mentioned to produce this *malaise*. But if, after relatively short periods of trial, the discomfort passes away, and adaptation becomes complete, then the peripheral side of its production may be of comparatively little importance. Experiments conducted in this laboratory seem to indicate that the former may occur when the unusual stimulus is a glaring light, and the latter more often occurs when the unusual stimulus is a sound, unless the sound is of excessive intensity. While we have little belief, on the whole, in the reliability of attempts to measure glare discomfort, or any other type of discomfort, in terms of subjective estimates (see *e.g.* (4)), it is tempting to imagine that in cases where it might be demonstrated that peripheral processes play an important part in its production, discomfort might be partially measured in terms of the specific physiological mechanisms involved. But all this requires much additional and more detailed investigation.

VII. SUMMARY.

Experiments are described in which observers were asked to call out the number of dots in patterns exposed successively at controlled speeds, the apertures at which exposure was made being so arranged that the observer had to move his eyes rapidly from one window to another

horizontally through an arc of 60 degrees. The observations were carried out both with and without glare. When a speed of exposure was arranged at which an observer, working under normal lighting conditions, could carry out this task without noticeable strain, the introduction of glare invariably produced subjective discomfort and in most cases extra effort. There was then no work decrement. When the speed of exposure was the maximum consistent with perfect performance under normal lighting conditions, the introduction of glare invariably produced marked decrement of work, and discomfort might or might not be reported. If the speed was still further increased further deterioration of work was produced, but the difference between the efficiency of work under normal conditions and glare conditions was less. These results are discussed.

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(Manuscript received 12 June, 1932.)

LOCALIZATION OF SOUND IN THE ANTERIOR-POSTERIOR AND VERTICAL DIMENSIONS OF "AUDITORY" SPACE¹

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- I. *Introduction* (pp. 320-1).
- II. *Review of related studies* (pp. 321-5).
- III. *Experimental method* (pp. 325-6).
- IV. *Experiment I: localization of tones in the quadrant and inter-quadrant positions* (pp. 326-7).
- V. *Experiment II: the influence of frequency on localization in the vertical dimension* (pp. 327-8).
- VI. *Experiment III: localization in the vertical dimension* (pp. 328-30).
- VII. *Experiment IV: localization in the anterior-posterior dimension* (pp. 330-2).
- VIII. *Summary and interpretation of results* (pp. 332-3).
- References* (p. 334).

I. INTRODUCTION.

AUDITORY SPACE, as used here, refers to the space occupied by and immediately surrounding the head of the observer. This space is so defined because it is the space into which the observers used in the present investigation normally projected the phenomenal sounds. The dimensions indicated for this space are psychological as opposed to the conventional geometrical dimensions. They refer to directions as perceived (1, p. 216 ff.). Anterior-posterior refers to directional localization in relation to the vertical plane of the aural axis. Vertical refers to directional displacement from the horizontal plane of the aural axis. In some cases these directions are referred to as *quadrant* and *inter-quadrant* localizations, which means that in such cases the perceived directions are considered as grouped according to the quadrants as defined by the intersection of the horizontal and the vertical planes of the aural axis.

There is considerable evidence in the earlier literature of sound localization that the perception of direction in relation to the vertical

¹ The investigation here reported was carried out in the Laboratory of General Psychology in Harvard University during the year 1930-1, at which time the writer was a National Research Council Fellow.

plane may be a function of absolute intensity or wave form, or of both of these factors operating together¹. Certain recent studies (3, 4, 5) indicate that localization in relation to both the vertical and the horizontal planes, but especially the horizontal plane, may be a function of frequency or wave form. Also there is some evidence here and there throughout the literature that localization in the anterior-posterior and vertical dimensions depends upon auditory habits or attitudes peculiar to the observer².

The purpose of the present investigation was further to define these factors as conditions for effective sound localization³. Additional work in this direction seemed justified, research in the field having become highly specialized in the light of specific, conflicting theories of localization.

According to the three leading theories of sound localization, viz. the intensity-theory, the phase-theory and the time-theory, sounds are localized consistently on the basis of binaural differences in one or another of the different factors (8), but they are so localized only in relation to the medial plane. In other words, auditory space, according to these theories, is primarily uni-dimensional, linear from right to left. However, it is a common observation in the laboratory and outside that sounds are localized also in relation to the horizontal and the vertical planes, i.e. as high, low, or between these directions, and as in front, in the rear, or between these positions. If sounds were localized only in relation to the medial plane, they would always be localized on some extension of the aural axis, which is the case only under certain conditions.

In the last analysis auditory space-perception may not be entirely explicable on the basis of response to binaural differences in acoustic intensity, wave-phase, and the time of arrival of corresponding wave-fronts. It may be that the influence of factors of a different relative as well as absolute order will require consideration.

II. REVIEW OF RELATED STUDIES.

Of the earlier studies bearing directly on the problem under consideration, the investigations by Seashore (9), Pierce (2), and Starch (10) are

¹ The evidence here referred to is given in summary-form by Pierce (2).

² The writer has summarized the evidence on this aspect of localization in a previous study (6). For a comprehensive interpretation of the evidence referred to here see (7).

³ The probable significance of further work in the direction of determining the nature and the conditions of localization in the anterior-posterior and vertical dimensions was identified in the course of an attempt, made by the writer, to determine the conditions most favourable for an experimental analysis of the relative rolls of wave-phase and acoustic intensity in sound localization.

outstanding. In these investigations auditory space was explored by shifting about the sound source, or sources in some cases, in relation to the observer, who was instructed to indicate the direction from which the sound appeared to come in each case. The conditions of experimentation closely approximated the normal conditions of hearing, as no attempt was made to isolate and control any particular aspect of the stimulus-pattern.

In the study by Seashore, it was found that the observers constantly tended to misplace the sounds upward in relation to the horizontal plane of the aural axis, and also to misplace the sounds forward in relation to the vertical plane, although the latter tendency was not so strong (9), p. 53). Seashore explained these tendencies as follows: "In ordinary experience we hear more sounds from the region of the horizontal plane through the ears, for the reason that this space is larger and contains more sources of sound than the lower region. We also hear more sounds from the objects that we pay attention to, *i.e.* face, than from objects we do not attend to, *i.e.* those behind us. Hence we normally expect more sounds to come from 'up front' than from any other directions" (p. 54). Thus Seashore tends to explain localization in the anterior-posterior and vertical dimensions on the basis of 'expectancy' or auditory habit. However, Seashore found also that his observers tended to place the weak sounds toward the rear and the strong sounds toward the front, which indicates, as Seashore pointed out (p. 54), that localization in the anterior-posterior dimension is influenced by the intensity of the binaural stimulus-pattern.

This work by Seashore was done under conditions in which all of the different aspects of the stimulus-pattern were controlled relatively to the two ears, because the sound sources were in the medial plane, so that in effect he was investigating the effectiveness of absolute factors as modified by head shadows and the external ear.

Pierce found, in the study referred to above(2), that his observers tended to localize relatively weak sounds behind and relatively strong sounds in front (p. 91). He found also that sounds coming from the same source but from a different position in relation to the head of the observer were qualitatively different (p. 90). These facts indicate that localization in the anterior-posterior and vertical dimensions may be influenced by absolute intensity and wave-form. Pierce found further that training in the discrimination of directions in the medial plane materially improved the ability on the part of his observers to localize sounds in this plane (p. 88). He concluded that the ability to localize sounds in unusual positions is a product of experience. But he qualified this position with

the statement that, "If the exigencies of life required, an ability to localize sounds perfectly within the medial plane [*i.e.* in the anterior-posterior and vertical dimensions] could be acquired" (p. 103). This implies that there must be certain aspects of the auditory stimulus to which learning in this direction can be attached.

Starch in his investigation (10) thoroughly explored auditory space in the anterior-posterior and vertical dimensions as well as also in the lateral, *i.e.* right-left, dimension. He found that: "In the horizontal planes, localization is most accurate in front, nearly as accurate in the back, and least accurate at the sides. In the vertical planes, excluding the medial plane, localization is most accurate above and below, and least accurate at the side. In the medial plane, localization is less accurate than in any other plane; it is more accurate in the anterior than in the posterior half" (p. 44).

The fact that the direction from which sounds come can be discriminated with any degree of accuracy in the different planes is evidence that certain physical factors must be operative. Starch concluded by way of interpretation that localization depends chiefly upon binaural intensive ratios; but that monaural ratios (absolute intensity), pitch (frequency), *timbre* and richness (wave-form), and other factors play important rôles.

Recent studies bearing directly on the present problem include two investigations by Young (3, 4), and one by Pratt (5).

In his first study (3), Young investigated the ability to localize sounds when the stimuli which, under normal conditions, would have arrived at one ear were led to the other ear by means of a reversing pseudophone. Under these conditions Young found that auditory localizations in the right-left dimensions were reversed; that localizations in the anterior-posterior dimensions tended to be thrown toward the back (p. 422); and that localization in the vertical dimension tended to be indefinitely localized or unlocalized (p. 412). By way of interpretation, Young states that, "The physical theories are doubtless correct in maintaining that sound localization depends upon some difference between the stimulation of the right and left ears; but these theories do not go far enough. They throw no light on the physiological and psychological processes of localization. They fail to take into account the possibilities of visual and auditory-motor localizations" (p. 424 f.). He concludes finally that, "Localization is believed to be a function of the total organism, accomplished in various ways, but involving a neuro-muscular adjustment with respect to a source or to a total spatial situation" (p. 429).

Young's second study(4) is an extension of the first. The external ears were detached by means of tubes and trumpets and extended to a frame of reference independent of the observer, so that head movements could not change the conditions of the stimulus. Under these conditions Young found that right-left localizations could be successfully discriminated, but that reliable tri-dimensional localizations were impossible. Judging from Young's position as stated above, from his first study, he would probably interpret these facts in terms of "neuro-muscular adjustment"; but it is possible to consider that head movements operate to change the absolute intensity and the wave-form of the stimulus-pattern, thus forming a basis for discrimination. Or, in other words, sounds in different positional relationships with the head may have positional characteristics due to the modification of the wave-form and absolute intensity by the corrugations of the external ears and by head shadows.

Pratt's study(5) deals directly with the problem of localization in the vertical dimension. The observers were instructed to indicate the position on a vertical scale $2\frac{1}{2}$ metres in height, divided into 14 equal units, of each of five different tones, 256~, 512~, 1024~, 2048~, and 4096~, presented in haphazard order in five different positions on the scale, under conditions such that the observers could not identify the actual position of the source. The observers were made aware of the fact that the sound source was being shifted up and down, so that they might not tend to judge position on the basis of pitch. In the test series, the tonal series was presented ten times to six different observers. The observers uniformly judged the higher tones to be higher on the scale than the lower tones. In fact, on the basis of averages, every observer placed the tones in order, from high on the vertical scale to low, according to the frequency order of the tones, highest frequency first, and so on down. From the data obtained from the test series and certain experiments made for the purpose of checking the original results, Pratt concluded that, "High tones are phenomenologically higher in space than low tones" (p. 283). In the light of this conclusion, the inability to localize sounds in the vertical dimension under normal conditions is not difficult of explanation, because in a normal environment sounds are distributed in haphazard order as to pitch and space.

However, it is possible to interpret Pratt's results, as Pratt himself suggests but does not believe (p. 283 f.), in the light of habits and ability in pitch discrimination, in which case localization would be a matter of discrimination in a pitch continuum rather than discrimination in a spatial continuum.

It is possible to interpret Pratt's data in still another manner. It might have been that the tones Pratt used as stimuli differed in richness of partials, or rather in the prominence of partials from the lowest tone which was the most complex to the highest tone which was least complex. If so, localization in the vertical dimension according to the pitch continuum could be explained on the basis of wave-form or phase in some other than the sine dimension. The fact that sounds from the same source but in different positions in relation to the head appear to be different in quality and intensity lends support to this interpretation; and so does the fact that the ability to localize sounds in the medial plane can be improved by training when only auditory cues are consistently present, because learning could not take place if there were no aspects of the stimulus-pattern to which the learning could be attached.

As for the evidence that localization in the anterior-posterior and vertical dimensions depends upon auditory habits or attitudes peculiar to the observer, nearly all of the more recent investigations dealing with localization in the right-left dimension refer to the fact that under the conditions of experimentation localization in dimensions other than the right-left is indefinite and uncertain; and investigators generally suggest that effective localization in such dimensions perhaps depends upon knowledge of the environment, auditory habits, or momentary attitudes¹. But the experimental conditions in these investigations may have been such as to break down the normal frame of reference and emphasize only the right-left aspects.

III. EXPERIMENTAL METHOD.

The problem under consideration was approached in a series of four different experiments dealing with different specific aspects of the problem. The first experiment dealt with localization as influenced by auditory habits, while the second experiment was concerned with the influence of absolute frequency and absolute intensity on quadrant and inter-quadrant localization. These two experiments were performed under the usual conditions of bi-lateral stimulation, in which short tonal impulses were presented in haphazard order. The third experiment attacked the problem of localization in the vertical dimension as a function of frequency, wave-form, or phase in some dimension other than the sine dimension. The fourth and last experiment dealt with the problem of localization in the anterior-posterior dimension as a function of absolute

¹ See p. 321, n. 2.

intensity. In the last two experiments, the conditions of observation were modified in that the total stimulus-pattern was varied continuously over a relatively wide pitch-range in one case and a relatively wide intensity-range in the other.

The set-up for these experiments consisted of an audio-oscillator¹ for the generation of the tones, together with accessory apparatus which made it possible to present the tones by the open-air method, to the observers (individually) in a quiet room. The receiving phones were held at a constant distance (15 cm. respectively) from the ears of the observers throughout the series of experiments.

The observers² were graduate students and members of the staff in the Department of Philosophy and Psychology in Harvard University. These observers did not consider themselves as trained observers, although they were all more or less familiar with the facts and theories of sound localization. In the early stages of each experiment the observers were asked to report orally on their perceptual experiences. Later, more specific instructions were given, depending on the nature of the experiment.

IV. EXPERIMENT I: LOCALIZATION OF TONES IN THE QUADRANT AND INTER-QUADRANT POSITIONS.

If localization in the anterior-posterior and vertical dimensions depends on auditory habits peculiar to the observer, it should turn out that each observer favours a certain particular quadrant or inter-quadrant position in his localizations. The purpose of this experiment was to test this possibility. Tones of various frequencies ranging from 500 ~ to 3950 ~ and of various intensities ranging from very weak to very strong were presented in haphazard order. The observers were instructed to indicate the localization in each case as 'down front,' 'front,' 'up front,' 'up,' 'up back,' 'back,' or 'down back.' Approximately 250 localizations by each of the five observers were obtained under these conditions.

Table I presents the data in terms of the percentages of the localizations falling in each quadrant or inter-quadrant position for each observer. It is obvious from these data that the observers individually tend to favour a particular quadrant or inter-quadrant position. This seems to justify the conclusion that localization in relation to the horizontal and

¹ The audio-oscillator used in the present study was developed and used by Pattle. For a description see (11).

² Mr J. Volkmann, Mr H. Cantrill, Dr C. Goldthwait, Mr W. A. Hunt, and Mr W. A. Bousfield served as observers in this study.

Table I. *Quadrant and inter-quadrant localization of tones.*

Obs.	Positions						
	D.F.	F.	U.F.	U.	U.B.	B.	D.B.
<i>B</i>	1*	7	92	—	—	—	—
<i>C</i>	—	—	1	—	71	3	25
<i>G</i>	—	—	1	4	95	—	—
<i>H</i>	—	—	—	—	41	45	14
<i>V</i>	—	—	1	16	67	6	10

D.F.=down front; F.=front; U.F.=up front; U.=up; U.B.=up back;

B.=back; D.B.=down back.

* The values in this table are percentages, based on approximately 250 localizations for each observer.

the vertical planes depends upon auditory habits; and such may be the case in everyday life where the tendency is to depend on vision and a knowledge of the environment and sound sources. But it will be observed also in Table I, that the observers individually tend to vary their localizations over a considerable range of quadrant and inter-quadrant positions. This fact suggests that localization in the anterior-posterior and vertical dimensions, even though determined very largely by auditory habits, may be influenced by other factors such as absolute intensity, frequency, wave-form, or momentary attitudes.

V. EXPERIMENT II: THE INFLUENCE OF FREQUENCY ON LOCALIZATION IN THE VERTICAL DIMENSION.

The purpose of this experiment was to determine whether variations in frequency explain in part at least the variability in quadrant and inter-quadrant localizations. Nine different tones, 500, 600, 700, 850, 1200, 1750, 1900, 2250, and 3950 cycles, were presented to the observers under the conditions of the previous experiment, except that attempt was made in this experiment to control the absolute intensity of the tones so that they would be equal. This attempt, however, was not entirely successful, because even when the energy output of the oscillator was equated for the different tones, the higher tones still appeared to be more effective than the lower tones. Absolute intensity was still operative but presumably not in the usual manner.

The observers were instructed to indicate the angular displacement and also the direction of the tones from the horizontal plane.

The data are summarized in Table II. It is clear from these data that there is a strong tendency for the higher sounds to be localized higher in space than the lower sounds. This tendency is especially strong for all of

Table II. *The vertical localization of tones of different frequency.*

Obs.	Frequencies (d.v.)								
	3950	2250	1900	1750	1200	850	700	600	500
<i>B</i>	45 ± 14*	40 ± 13	40 ± 19	50 ± 11	42 ± 10	27 ± 9	31 ± 22	46 ± 20	42 ± 24
<i>C</i>	65 ± 10	54 ± 15	50 ± 5	52 ± 8	50 ± 10	31 ± 12	25 ± 10	18 ± 13	9 ± 12
<i>G</i>	79 ± 10	74 ± 10	73 ± 7	71 ± 12	70 ± 10	60 ± 19	56 ± 18	51 ± 8	47 ± 13
<i>H</i>	62 ± 8	34 ± 21	27 ± 15	38 ± 24	36 ± 18	13 ± 19	18 ± 12	6 ± 26	-17 ± 14
<i>V</i>	86 ± 8	82 ± 9	80 ± 11	84 ± 7	73 ± 16	68 ± 12	60 ± 19	50 ± 23	21 ± 13

* These values are averages with average deviations of ten estimates of the angular displacement of the tones from the horizontal plane of the aural axis. The positive values represent displacements above, and the negative values below, the horizontal plane.

the observers except *B*. The differences in angular displacement between the lowest and the highest tones is 56 for observer *C*, 32 for observer *G*, 79 for observer *H*, and 65 for observer *V*, with the intermediate tones localized between the extremes in each case, and almost uniformly high to low in space according to the pitch series. *B*'s observations for the different frequencies are variable but the variations are inconsistent. The localizations by all of the observers tended to fall into the quadrant and inter-quadrant positions indicated for each observer in Table I.

Evidently frequency or some correlative aspect of frequency, perhaps wave-form, operates to influence localization in the vertical dimension. Absolute intensity may have been sufficiently effective to determine the variability in the anterior-posterior dimensions. However, there remains the possibility that the variability of the localizations may have been influenced by momentary attitudes.

The results may be taken as supporting Pratt's position that high tones are phenomenologically higher in space than low tones. But the present writer inclines to the position that localization in the vertical dimension may be a function of wave-form or phase in some other than the sine dimension; because it was easily observable that the lower tones used as stimuli in the present investigation were more complex than the higher tones.

VI. EXPERIMENT III: LOCALIZATION IN THE VERTICAL DIMENSION.

The purpose of this experiment was to determine further the rôle of frequency (or wave-form, assuming that the difference in the complexity of the tones may be the effective factor) in localization in the vertical dimension. In this experiment the frequency of the total binaural stimulus-pattern was varied over a series of frequencies, and in such a manner that each frequency followed the next above in the frequency

continuum (or the next below, depending on the direction in which the series was varied) in rapid succession. The series included the following frequencies: 1200, 1250, 1300, 1400, 1500, 1600, 1750, 1900, 2150, 2900, and 3950 cycles. This series was presented in both ascending and descending order and under conditions of different binaural intensive ratios, according to chance, so that the observer could not know in which direction the series was to be presented nor what particular position in relation to the medial plane would be the starting-point. The binaural intensive ratios were such that at one time the apparent source would be at the extreme lateral position, at another time in the medial plane region, and again midway between these positions.

The observers were directed to record the angular displacement of the apparent source from the horizontal plane both at the beginning and at the end of the series. They were instructed also to draw on a chart before them the course made by the phantom sound.

Table III summarizes the data as to localization in the vertical dimension under the conditions of the experiment. Again it is obvious

Table III. *Localization in the vertical dimension as determined by a series of frequencies each presented in rapid succession, under conditions of different binaural intensive ratios.*

Obs.	Binaural intensive ratios*					
	1 : 59		1 : 3		1 : 1	
	1200→3950	Range†	1200→3950	Range	1200→3950	Range
B	14†→31	17	33→47	14	22→49	27
C	18 →80	62	15→79	64	16→79	63
G	24 →47	23	47→61	14	60→65	5
H	-21 →61	82	-13→50	63	-14→74	88
V	13 →61	48	51→80	29	85→88	3

* Intensive ratios are given in terms of the resistances in the electric circuits.

† The frequency series consisted of: 1200→1250→1300→1400→1500→1600→1750→1900→2150→2900→3950 ~.

‡ The values in this table are averages of ten estimates of the angular displacement of the fused sound from the horizontal plane of the aural axis, at the beginning and at the end of the series. Positive values represent displacements above the horizontal plane; negative values below. The range values of course represent the difference between the displacements at the beginning and at the end of the series. The arrows represent direction of change.

that high tones tend to be localized higher in space than lower tones. The range of variation is different for the different observers, and also for each observer under the different intensive ratios. But uniformly the observers indicate a corresponding change in the localization of the phantom source as the frequency of the stimulus-pattern is changed.

Under the conditions of this experiment, the observers uniformly reported apparent movement of the phantom source, or the appearance of successive apparent sources, in an arc originating in a position lower in space for the lowest tone and ending in a position higher in space for the highest tone. The 'arc-effect' as described by the observers corresponds very much in general characteristics to the 'phase-, 'time-, or 'intensity-effect' in right-left localization.

In the beginning stages of the experiment the observers were not always able to identify the general direction of the change in the localization of the apparent source. One observer, *H*, reported that as the series was presented in either direction the whole spatial frame of reference seemed to shift. Another observer, *G*, reported that when he attended strictly to the sound it appeared to spread without taking any definite localization, but that when he attended to the localization, movement, *i.e.* the arc-effect, was apparent. All of the observers reported that at times the "middle of the course" was blurred, because the phantom appeared to make its course so rapidly.

The results seem to justify the conclusion that absolute frequency, or some physical correlate of it, operates under conditions of continuous or serial change to condition localization in the vertical dimension very much as continuous or serial variations in phase, intensity, or time condition corresponding 'effects' in the right-left dimension.

VII. EXPERIMENT IV: LOCALIZATION IN THE ANTERIOR-POSTERIOR DIMENSION.

The purpose of this experiment was to determine the rôle of absolute intensity in localization in the anterior-posterior dimension. The conditions of observation were the same as the conditions of the previous experiment, except for the fact that the absolute intensity of the stimulus-pattern was used as the variable factor, while the vertical plane of the aural axis was used as the plane of reference. Also only one tone (1200 ~) was used as stimulus in this case.

The results are summarized in Table IV. One of the observers, *G*, is not represented in this table, because in the test series he reported that the localizations were vague or that he could not identify any change in the spatial characteristics of the tones during the presentations, beyond the fact that the tones appeared to vary in volume and sometimes in distance. However, in the preliminary observations this observer did report 'movement.' The other observers reported fairly consistently that

Table IV. *Localization in the anterior-posterior dimension as conditioned by continuous variation in the absolute intensity of the total stimulus-pattern, under conditions of different binaural intensive ratios.*

Obs.	Binaural intensive ratio*					
	1 : 59		1 : 3		1 : 1	
	W→S†	Range	W→S	Range	W→S	Range
B	18 F.†→67 F.	49	90 F.→64 F.	26	90 F.↔90 F.	0
C	65 B.→8 B.	57	70 B.→34 B.	36	90 B.↔90 B.	0
H	50 B.→0	50	0↔0	0	90 B.↔90 B.	0
V	62 B.→4 B.	58	53 B.→37 B.	16	0 U.↔0 U.	0

* Legend same as in Table III.

† The intensity was changed during each observation, from very weak to very strong, or the reverse.

‡ Read the table as: 18 degrees in front of the vertical plane of the aural axis to 67 degrees in front, covering a range of 49 degrees (first values for observer B), etc. The values are averages of approximately ten estimates of the angular displacement of the apparent sound-source in the front-back dimension, from the vertical plane of the aural axis.

the phantom source seemed to move in an arc from a position toward the front to a position toward the back when the intensity of the pattern was varied from very strong to very weak, and in the reverse direction when the intensity variation was from very weak to very strong. The phantom-sound was very often described as "coming down, moving toward the back, and approaching the medial plane" all at the same time, when the intensity was varied from very strong to very weak. The reverse of this was the case when the intensity was varied from very weak to very strong. However, the observers were not always able to identify the direction of the 'movement' (the aspect of distance seemed to interfere), and sometimes they would report only changes in volume.

From the data presented in Table IV, it is clear that when the phantom source was originally localized at the extreme lateral position (under the condition of a binaural intensive ratio of 1 : 59), the 'arc-effect' was greater in extent than at the intermediate position, and that the 'arc-effect' had played out entirely when the apparent source was localized originally in the medial plane. Under the conditions of a 1 : 1 binaural intensive ratio the phantom source shifted only as to distance, the apparent source being projected farther and farther from the original position when the intensity was varied from very strong to very weak.

The fact that the observers were somewhat variable in their reports on the 'arc-effect' indicates either that the experience was extremely complex, or else that localization under the conditions of the experiment is influenced to a considerable extent by the attitudes of the observer.

However, the results seem to indicate that localization in the anterior-posterior dimension may, to the degree that it depends on auditory cues, be a function of the absolute intensity of the stimulus-pattern.

VIII. SUMMARY AND INTERPRETATION OF RESULTS.

The results of the present study may be summarized briefly and interpreted as follows:

1. Observers tend individually to localize sounds (tones) in a particular quadrant or inter-quadrant position; but the localizations for a given observer cover a considerable range of quadrant and inter-quadrant positions.

These facts indicate that habits of space-perception or 'expectancy' are important conditions in sound localization—conditions which may operate to overcome the 'effects' of primary auditory data for localization in quadrant positions other than the particular position favoured by the observer, assuming, of course, that such primary data are given. The fact that the localizations vary over such a range of positions in any case may indicate that differences in frequency and absolute intensity of the stimulus-pattern, and momentary attitudes of the observers, play significant rôles in the localization of sounds.

2. High tones are localized higher in the range of the quadrant and inter-quadrant positions than low tones; loud tones appear to be localized toward the 'front' positions, and weak tones toward the 'back' positions.

These facts tend to the conclusion that absolute frequency and absolute intensity are important conditions for localization in the anterior-posterior and vertical dimensions—conditions that might be adequate for localization but for the stronger influence of auditory habits, and perhaps momentary attitudes.

3. Under conditions of continuous or serial change in the frequency of the binaural stimulus-pattern, the apparent source describes an arc about the head, from a point lower in space for the lower tones to a point higher in space for the higher tones, when the change in frequency is from low to high. The phenomenon is reversed when the frequency change is reversed in direction.

This phenomenon is of the order of the phase-, time-, and intensity-'effects' in right-left localization; and may prove to be of primary importance in the study of localization in the vertical dimension.

Whether this phenomenon is conditioned by frequency variation or by wave-form is in question. If it is conditioned by frequency alone, then

auditory localization in the vertical dimension cannot be 'effective' for orientation, since high-pitched sounds would be localized high in space, and low-pitched sounds low in space, whether the respective sources were high or low, or not. On the other hand, if the phenomenon is conditioned by wave-form, it could be accounted for on the basis of differences of wave-form which are determined by the position of the sound source in relation to the head. The writer inclines to the latter interpretation, but the precise wave-form differences which are effective in this way need further investigation.

4. Under conditions of continuous variation in the absolute intensity of the total stimulus-pattern the phantom source tends to move in an arc from a position to one side of the head and forward, to a position near the medial plane and 'back,' when the intensity is varied from very strong to very weak. The reverse is the case when the intensity change is reversed. This 'absolute intensity effect' is not very pronounced. Perhaps it would be more to the point to say that this 'effect' was not reported by all of the observers used in the present investigation, and that the observers who did report it, did not always report it. However, this inconsistency of report may have been due to the fact that the tonal experience was too complex for attentional analysis. That the experience was very complex is indicated by the fact that the observers often reported changes in quality, volume, and distance accompanying the changes in intensity.

In any case it seems that 'absolute' intensity may be an important condition for localization in the anterior-posterior dimension, the condition of 'absolute' intensity being modified or determined for the different positions relative to the head and the vertical plane, by head shadows and the pinnae.

5. In general the results indicate that any adequate theory of sound-localization will have to explain localization in the anterior-posterior and vertical dimensions together with localization in the right-left dimension.

It may be that auditory localization, *i.e.* localization depending on auditory cues, can be explained on the basis of 'effects' corresponding to the different relative and absolute aspects of the total binaural stimulus-pattern. In this case perhaps some particular characteristic of the wave-form accounts for localization in the vertical dimension, absolute intensity for localization in the anterior-posterior dimension, and the relative factors, *i.e.* binaural difference factors, for localization in the lateral or right-left dimension.

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(Manuscript received 5 May, 1932.)

A NOTE ON CORRELATIONS

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- I. *Introduction* (pp. 335-6).
- II. *Application to correlations for factor studies* (pp. 336-7).
- III. *Derivation of the rank method ρ* (pp. 337-8).
- References* (p. 338).

I. INTRODUCTION.

It is perhaps not sufficiently appreciated that the product-moment correlation coefficient can be calculated from crude scores, using 'differences' instead of the more cumbersome 'products.' Kelley⁽¹⁾ draws attention to this 'difference' method of working correlations, but its special use in work that requires the calculation of large numbers of correlations for the purpose of critical factor studies may excuse its present reiteration.

The expression for r , calculated from crude scores with zero as arbitrary mean for each variate, is as follows:

$$r = \frac{\Sigma XY - nM_1M_2}{\sqrt{\Sigma X^2 - nM_1^2} \sqrt{\Sigma Y^2 - nM_2^2}} \quad \dots\dots(1)$$

(where M_1 and M_2 are the *true* means of the X and Y variates).

Now by expanding $\Sigma (X - Y)^2$, we have

$$\Sigma XY = \frac{1}{2} (\Sigma X^2 + \Sigma Y^2 - \Sigma (X - Y)^2).$$

Thus, (1) may be written:

$$r = \frac{\Sigma X^2 + \Sigma Y^2 - \Sigma (X - Y)^2 - 2nM_1M_2}{2\sqrt{\Sigma X^2 - nM_1^2} \sqrt{\Sigma Y^2 - nM_2^2}} \quad \dots\dots(2).$$

This is the 'difference' expression for r , its advantage, under certain circumstances, lying in the use of $(X - Y)$ instead of XY .

When a large number of variables have to be intercorrelated, especially if some form of adding instrument is available, the 'difference' expression is particularly economical of working time if the following calculational procedure is used:

(a) The frequency distributions of the crude scores for each variable are drawn up.

(b) Now consider any one variable, such as, say, the crude scores obtained by a given population in a certain mental test. The frequency distribution for this variable, if it allows of a little 'smoothing,' usually suggests that a more convenient scale may be used in the place of the crude one. The new scale might conveniently be one of 20 points, or 15 points, ranging from 0 to 19, or 0 to 14 respectively.

(c) Let all the variates be converted to such 'scaled' scores. The values ΣX^2 , and the means $\Sigma X/n$, for the 'scaled' scores are readily obtained from their frequency distributions.

(d) The values $\Sigma (X - Y)^2$, for each pair of variables, are got from the individual 'scaled' scores in order. These differences are themselves all integers, usually ranging from 0 to 10 or so for correlations between mental tests.

(e) If a new arithmetical table is learnt, in which it is remembered that $3^2 + 1^2 = 10$, $3^2 + 4^2 = 25$, $6^2 + 8^2 = 100$, etc., the summed value $\Sigma (X - Y)^2$ is worked with some ease.

The above procedure is simply a scale conversion, whereby the crude scores for each variable are rescaled to fit a scale which is amenable to rapid working and easy checking, for the 'difference' form of the product-moment coefficient. The crude scores should always allow of this re-scaling with a little smoothing, alike in the interests of reliability and of comprehensiveness of the variables.

II. APPLICATION TO CORRELATIONS FOR FACTOR STUDIES.

If the frequency distributions are identical for all the variables, so that ΣX^2 and M are the same for all the correlations, expression (2) reduces to (3) below:

$$r = 1 - \frac{\Sigma (X - Y)^2}{2(\Sigma X^2 - nM^2)} \quad \dots\dots(3).$$

Or, shortly, where ' k ' is a constant for all the correlations for the given population and identical distributions,

$$r = 1 - \frac{\Sigma d^2}{k} \quad \dots\dots(4).$$

The expression (4) is suggested for use when there are very many intercorrelated variables, for large populations. The rescaling necessary for use of (4) could follow the lines suggested in the previous section,

except that the same new scale would be used throughout, with identical frequency distributions, for all the variables. Throughout, the crude scores should allow of coarse grading to accommodate the new scale. This procedure was followed in papers by the present author(2) and by Dr Wm. Brown and the present author(3). The time expended on the initial rescaling is more than offset by the saving in time in using expression (4): the larger the number of variables, of course, the greater is the saving and convenience.

But apart from the economy effected in the calculations, the use of (4) has other considerations in its favour. It is essential for factor studies that the correlation surfaces of the pairs of variables should be comparable. This is the case when each is homoscedastic, homoclitic, with rectilinear regression lines. Thus all factor studies for large populations should begin, in our opinion, with a report of data establishing the comparability of the correlations (and incidentally of the P.E.'s) from the point of view of their correlation surfaces. It was found(2) that the above rescaling procedure ensured more exact comparability of correlation surfaces, and that it was a necessary condition for accurate tests of the two-factor theory. The rescaling 'smoothes' the crude data, but studies of reliability show that, with the wide range of intervals suggested at (b) above, the smoothing only removes specificity(2).

III. DERIVATION OF THE RANK METHOD ρ .

Spearman(4), in his early paper on the method of rank correlations, stated the identity of the product-moment expression and that for rank method (which, of course, is not to be confused with the very different Spearman 'footrule' R). A simple derivation of ρ follows from (3) above, which, useful for lecture and demonstration purposes, also stresses the identity. Their non-comparability, nevertheless, is seen from the fact that Pearson's correction has to be applied to ρ to reduce it to r .

The derivation of ρ is as follows:

The ranks from 1 to n may be regarded as scores on an n -point scale, frequency 1 in each interval. Now apply expression (3) to any two ranked variates.

ΣX^2 's and M 's are the same, respectively, for both variates.

$$\begin{aligned}\Sigma X^2 &= 1^2 + 2^2 + 3^2 + \dots + n^2 \\ &= \frac{1}{3}n(n+1)(2n+1).\end{aligned}$$

M , the mean of the first n integers, is $\frac{1}{2}(n+1)$.

That is, $(\Sigma X^2 - nM^2) = \frac{1}{12}n(n^2 - 1)$.

Substituting in (4) then gives:

$$r = 1 - \frac{6\sum d^2}{n(n^2 - 1)} \quad \dots\dots(5),$$

which is the Spearman formula for rank method correlation.

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(Manuscript received 2 September, 1933.)

PUBLICATIONS RECENTLY RECEIVED.

Social Development in Young Children. By SUSAN ISAACS. London: George Routledge and Sons, Ltd. 1933. Pp. vii + 480. 15s. net.

It is over three years since *Intellectual Growth in Young Children* was published, as the first volume of a trilogy on the behaviour of little children, of which the book now under review forms the second part.

In Vol. II we are shown the same children from a different point of view; in Vol. I their intellectual development between the ages of 2 and 10 years was traced and discussed. Here the writer's business is with their social growth during the same period. The study has a decided Freudian bias, but is intended for a scientific public and is not meant to be a popular exposition. Its object is to open up enquiry in the vast field of problems with which it is occupied, and it does not pretend to say the last word on any of the questions involved. One of the most astonishing things in the book, therefore, is its author's apology (p. 399) for her 'partial and fragmentary' treatment of this immense subject; for despite its apparent bulk, it is the quality of balanced conciseness which will make the work such a welcome addition to any psychological library.

Like its predecessor, this book is based upon observations of the "spontaneous behaviour of children in the real situations of their daily social life." Its main aim is "direct qualitative study of the individual children's feelings and doings among their fellows," though, as is pointed out, many of the problems it raises, e.g. that of normality, are linked up with quantitative issues. It consists of two parts. The first and by far the larger part (pp. 1-400) is concerned with the psychological data. A brief introduction (chapter I) outlines the intention, plan and subject matter of the book, discusses the relative values of qualitative and quantitative records, and explains the writer's personal attitude to the problems she is about to examine. The original material in chapter II (pp. 30-204) covers the space of three years of group life. Chapter III (pp. 205-400) contains an analysis and discussion of the data supplied in the records. These two chapters provide the kernel of the book and should be read and considered in conjunction; they are complementary to one another and both conform to the same logical scheme.

The material and the discussion are arranged under two broad heads: (i) Overt social relations; love and hate in action; (ii) The deeper sources of love and hate. The first part of the records is filled with the material which provides data for the elucidation, in the third chapter, of the processes by which the child gradually develops out of his primary ego-centric attitude, passes through a phase of hostility and aggression, and then reaches the stage where some degree of friendliness and co-operation are possible for him. The questions of individual and group hostility are considered, and an able depiction and interpretation are given of the significance, for the total process of social development, of motives such as possession, power and rivalry, or feelings of inferiority and superiority or general anxiety.

Order and meaning are brought into what might on the surface appear to be a mere jumble of data by an appeal to psycho-analytic principles, deduced from studies according to a modified Freudian technique, of the unconscious mental life of both children and adults. Care is taken to point out, however, that any single piece of behaviour is always double-sided, with its conscious significance as well as its unconscious meaning, and that the emphasis on one aspect rather than another is placed in accordance with its relevance for the purpose at issue. As in her earlier book, the writer is very insistent about the experiential or functional nature of growth as opposed to the structural view adhered to by Piaget. From the evidence presented it seems fairly conclusive that social, like intellectual development, is essentially a

continuous process, in which the final product contains no elements which are new—i.e. none which were not potentially present in the primary matrix out of which all the others developed by experience of one sort or another. The significance of the original family group as a determinant of the design of the child's emotional growth is exemplified by the evidence and re-emphasized by the commentator.

Amid this whirl of facts and their interpretation, some of the wider applications to difficulties of adult social life are not lost sight of. Thus a discussion of the mechanism of projection, shown to have been at work in the behaviour of the Malting House School children, is used to illumine the problem of the moral aggressiveness of human society as a whole—"an unending tale of burning, maiming, killing, torturing, imprisonment... (and) cruel revenge."

In the sections headed *Friendliness* and *Co-operation* the writer reviews at some length the potency of environmental differences, and stresses the importance of the adult as a super-ego, or check upon the child's own disruptive tendencies. He is shown as an 'easer' of the child's inner tension because he is an aid to his being good. It cannot perhaps be too often pointed out, especially in these days of 'experimental' education, that a clear understanding of the sense of security and happiness which children gain from the wise leadership and control of a sympathetic adult, is of fundamental educational importance; for a little child is incapable of setting his own right limits and the absence of the correct degree of this limiting factor becomes the breeding ground of all kinds of serious anxieties and fears. There is an attractive account of the change in attitude, evident at this co-operative stage, in the children's behaviour towards younger children and new-comers. Restitutive action, the making of remorseful amends for hurts or hostility, becomes increasingly possible as knowledge grows and skill increases, and with the weakening of the grip of inner phantasies.

The presentation of the observable social relations among the children is followed by a study of the deeper underlying origins of their behaviour. To this section belong problems like those of sexuality in all its forms and phases from the oral to the genital, and the feelings of guilt and shame. To those who doubt the educational relevance of such material, it is retorted that the sexual interests of the children, from the point of view of general social control, should be, and at the Malting House School were, subjected to the same educational processes as their more open social relations and their emotional development in the wider sense. The child's notions of punishment and wrongdoing and his feelings of shame and guilt, or more strictly, the primitive forerunners of such feelings, those 'greater terrors' which are so real but which have so little relation to reality, are not, it is maintained, a product of educational experience or of environment as such, but are, on the contrary, a spontaneous development in the child's mind. They are "inherent in the fundamental interactions between human mental structure and inevitable early experience" (p. 371). The question is in fact another aspect of the growth of the super-ego.

The main section of Part II of the book (pp. 403-456) consists of a fairly exhaustive and extremely fertile explanation of the significance for everyday educational issues of the preceding genetic psychology of individual children. Two angles of the subject are presented. There is first a brief contrasting of the two processes of education and analysis, and an emphatic warning of the dangers which result from any confusion of the two spheres and functions. Then there is an exposition of the precise bearing upon educational aims and practice of the facts laid bare by the psycho-analytic technique. No entirely new principle emerges from this revelation, but, as might have been expected, an understanding of the facts of social and sexual development does lead to a revaluation of the different aspects of mental life as a whole, in favour of feeling and activity as opposed to mechanical learning or narrow intellectualism. It also points implicitly, though quite definitely, to the need for considerable correction and modification of some of the more extreme of recent theories (and practices) of educational 'freedom.'

Moral defect or delinquency is indicated by the findings of some of the child analysis as being the result not of a lack of inhibiting forces, but, paradoxically, of a surfeit of conscience of a too severe and primitive type. It is an attempt to "ease the internal pressure of a sadistic super-ego" by proving to one's self that it is safe to be bad.

Play is revealed as the means by which the child comes most easily to discover the world and which best brings him psychic equilibrium; the importance of giving educational opportunity for sublimation, by means of handicrafts and free creative activities like modelling, painting or acting, is made evident, while once again the immense value to the child of the companionship of his fellows is emphasized.

The frank imparting of sexual information when the need arises is shown to be both emotionally and culturally sound, because it helps to build up a joint basis of mutual respect and friendliness between adult and child and throws open to the restless interest of the latter the whole living world of biology. In some of the more specific problems of early education, of which verbal exhibitionism, whether sexual or excretory, may be cited as a particularly tiresome example, it seems clear that it is the adult in charge who, in a sense, holds the key position. Either licence or priggishness on his part is equally out of touch with reality, the one with social, the other with physiological fact. In order, then, not to fail badly in his function as a support to the growing child against his own guilt or incapacity, the adult must be on the side of "a reasonable conformity to general public standards."

The book throws light upon a number of other important difficulties which daily baffle the minds and fray the tempers of those who habitually work among children of every age. And in these enlightened days when, wisely or unwisely, parents and quasi-educators are so inclined to make anxious excursions into the scientific realms of child psychology, it would be well if they could be led, somehow, to understand the value of the sanity and balance with which evidence so explosively controversial can be marshalled.

The First Two Years. A Study of Twenty-five Babies. By MARY M. SHIRLEY.
Vol. II: *Intellectual Development*. Minneapolis: The University of Minnesota Press. 1933. Pp. xv + 513. \$3.00 net.

Vol. I of this important study was called *Postural and Locomotor Development*. The present volume records a mass of first-hand and reasonably controlled investigations of cognitive developments in young infants. Wherever possible specific tests were employed, and the observations were carried out upon children in their own homes, in most of the cases. What is called the 'follow through' method was used, that is to say, the children were kept under careful observation throughout the whole of the first two years of their life. It appears that the activities of young infants develop in an orderly sequence "independent of the rate of development and of the environmental conditions under which the babies live." Development proceeds from 'mass activity' to the level of specific acts. New reactions appear more rapidly during the first year than the second, but it is during the second year that the specific activities which have already appeared begin to become well organised. Many new acquisitions seem to emerge suddenly, and the early stages of growth are marked by considerable fluctuation. The book is a contribution to psychology of real value, and the material is presented in a lucid and interesting manner.

The First Two Years. A Study of Twenty-five Babies. By MARY M. SHIRLEY.
Vol. III. *Personality Manifestations*. Minneapolis: The University of Minnesota Press. Pp. xi + 228. \$2.50.

This volume completes Prof. Shirley's important first-hand studies of early development in the child. Although in bulk it is smaller than the others it is certainly not less interesting and not less valuable, and it is even more original in plan and achievement. Its conclusions concerning the very early and decided expression of personality, and the pervasiveness and persistence of personality traits, will not surprise any observant fathers and mothers. But these matters have received very little notice from the psychologist proper, and Prof. Shirley's third volume deserves a wide and a warm welcome. The observations, carefully continued for long periods,

bear a stamp of accuracy and of sympathetic understanding. The case studies are admirably presented, and their subjects appear throughout as persons and not merely as specimens. The Institute of Child Welfare at the University of Minnesota, and Prof. Shurley and her collaborators, are warmly to be congratulated upon the completion of a research well planned, well executed and well described.

The Evidence of our Senses. By A. W. P. WOLTERS. London: Methuen's Monographs on Philosophy and Psychology. 1933. Pp. vii + 85. 2s. 6d. net.

Nobody who has ever tried to write a general account of perceptual processes, to make it interesting, to miss out nothing of importance, to write intelligibly for the general reader without sacrificing significance for the trained psychologist, and to do the whole thing within the limits assigned to this book will underrate the difficulty of the task. Mr Wolters has tried and has succeeded, and the achievement is remarkable. An intelligent general reader will enjoy and understand the argument. The student approaching exact psychological study could have nothing better as an introduction to his work. To one reader at least who has spent not a few years largely in the specific study of problems of perception Mr Wolters' volume has given not only great pleasure but a valuable stimulation. Here and there perhaps a few overstatements have crept in: "the so-called distance senses are only symbolical"; "the observer who, taught by his previous errors, has acquired a healthy scepticism as to his perception, will probably detect at once [i.e. in the case of mal-perception] that *something* is different, and it is then a simple matter to detect what the difference is." These are debatable statements hardly to be affirmed with such emphasis. But taking it all through this is one of the best monographs of its type that has appeared in English for a very long time. It is now greatly to be hoped that Mr Wolters will proceed to explore other fields of investigation of mental processes. The style, the organization and the matter of this book are enough to show that he has a rather rare gift of exposition combined with a capacity for original thinking.

Studies in Expressive Movement. By G. W. ALLPORT and P. E. VERNON. New York: The Macmillan Co. 1933. Pp. 269. 15s. net.

So great a mass of writing is now being poured out upon tasks of diverse kinds that it would be a very easy matter for this book to escape serious notice. That would be a great pity, for it is a book which stands out from among many as worthy of the most careful attention. The authors are concerned with that method of studying personality which depends mainly upon an investigation of the consistency and organization of certain simple gestures and expressive movements. They planned and carried out an investigation of controlled simple gestures (using the word in a wide sense) in a group of diversified subjects. Here they present their analysis of the results, using throughout statistical measures with most admirable caution. The details of the conclusions must be studied in the book itself. As they are presented those conclusions appear to be both convincing and of far-reaching significance. The authors show in what senses and to some extent within what range consistency and constancy of expressive movement are to be found. They indicate the most closely related groupings of expressive movement. Especially interesting is their demonstration of the importance of what they call 'areal,' 'centrifugal' and 'emphasis' factors in movement, for this, as they show, indicates that "physical categories of movement are unsuitable models for the *psychological* study of expression."

Theories of expressive movement are considered both critically and in a constructive manner, and there is a very attractive and sympathetic study of what may be called 'scientific graphology.'

In sum, this is a good book of a fruitful kind which clears up many problems and points the way to a study of many more.

Psychology and the Choice of a Career. By F. M. EARLE. London: Methuen's Monographs on Philosophy and Psychology. 1933. Pp. vii + 103. 2s. 6d. net.

This book presents a balanced and judicious account of its important topic. The main subjects discussed are: abilities which may influence occupational success; choosing the best work; preparing to enter the career chosen; and psychological tests. Dr Earle has some particularly timely remarks to make on preparation for a career, in which he makes it clear that the mere diagnosis of occupational abilities must be supplemented by a well-arranged type of training. It is natural that in a book of this size and appeal the argument should for the most part run on a general plane. But sufficient illustrations are given to convince the most sceptical that psychology has an essential part to play in directing the choice of a career. Possibly something more might have been said about that provision and regulation of occupational opportunity which seems to be necessary if the psychological direction of careers is to be very efficient in actual life.

The Inventor and His World. By H. STAFFORD HATFIELD. Kegan Paul, Trench, Trubner and Co., Ltd. 1933. Pp. 269. 6s. net.

This very lively and interesting book is about equally divided between a practical, a theoretical and a descriptive treatment. In chapters dealing with The Practice of Invention, Working out and Financing an Invention, and Patent Law as it is and as it should be, the author conveys information which a good many would-be inventors seem to neglect to their considerable disadvantage. In discussions on What is Invention, The Inventor, The Direction of Inventive Effort, and Invention and the Transformation of Energy, many points of theory are considered, generally with considerable force and clarity. Other sections deal with mechanical, chemical, electrical, psychological and biological invention. These contain many illustrations of notable inventions in their respective fields, and make many suggestions concerning possible future inventions. The whole book is entertaining and, while it does not profess to provide any deep-going theory of inventiveness as a psychological function, it contains a large number of suggestions which have great psychological interest.

The Poor White Problem in South Africa. Report of the Carnegie Commission. Part. II. *Psychological Report.* By R. W. WILCOCKS. Stellenbosch: Pro Ecclesia-Drukkery, The Avenue. 1932. Pp. xxxiii + 206.

The Report is of absorbing interest and great value to the social psychologist anywhere in the world. For although the Poor White problem as dealt with here has many features peculiar to South Africa, yet almost exactly analogous problems affect the well-being and organization of nearly every large modern community. For the purposes of this study the Poor Whites are defined as persons of European descent who gained (or are still gaining) their livelihood mainly by farming. They are usually hired men on farms, owners of dwarf holdings, or of small undivided shares of land, poor settlers and unskilled workers now outside of farming. Their number has grown rapidly of recent years and has reached alarming proportions. The whole survey comprised Economic, Educational, Health and Sociological studies as well as this psychological investigation directed with great skill and industry by Prof. R. W. Wilcocks of the University of Stellenbosch. The psychological findings are here set forth with remarkable clarity and vigour and their implications discussed. It is impossible to summarize them even briefly without doing them an injustice. But the final conclusion of all is particularly striking having in view the sweeping statements often made about this and analogous problems. The actual investigation showed that the great majority of Poor Whites "are of normal intelligence," but suffer badly from unfavourable environmental conditions. "The greater part of them constitute a human material which need not be a burden but which may, granted a sound State policy, become a very decided asset to the Union." South Africa is not the only modern State which needs very seriously to consider these words.

The Intelligence of Scottish Children. A National Survey of an Age-Group. University of London Press, Ltd. 1933. Pp. x + 160. 5s. net.

On Wednesday the 1st of June, 1932, 87,498 Scottish schoolchildren of the age group $10\frac{1}{2}$ to $11\frac{1}{2}$ were given a group scholarship test arranged by Prof. Godfrey H. Thomson. Picture tests were included to help the pupils of lower grade, and subsequently 1000 of the children were individually tested by the Binet Scale. This massive undertaking grew out of a proposal made by Prof. James Dreyer and was organized by the Scottish Council for Research in Education. This book describes the experiment, and presents its main conclusions. These are:

That the average I.Q. of boys born in Scotland in 1921 is 100, and that of girls born in the same year is not significantly different.

That the I.Q.'s of the boys showed a wider scatter than those of the girls.

That the size of the groups above and below the average was greater than, from investigations elsewhere, appeared likely.

That not fewer than $1\frac{1}{2}$ per cent. and not more than 3 per cent. of Scottish children born in 1921 fall into a 'mentally deficient' class, and that this class contains considerably more boys than girls, these conclusions being dependent upon certain assumptions regarding 'verbal intelligence.'

The book cannot be adequately summarized in brief, however. It deserves careful and wide study, especially with regard to its far-reaching social implications.

Vorlesungen über die Arbeit der Grosshirnhemisphären. Von J. P. PAWLOW. *Autorisierte Übersetzung aus dem Russischen.* Von Dr G. VOLBORTH. Kniga (England, Ltd.), Bush House, London. 1933. Pp. viii + 480. 12s. 6d.

In 1924, Prof. Pawlow delivered in Russia a special course of lectures which attempted a complete and systematic study of his life-work on the functions of the cerebral hemispheres. These lectures are here translated into German and they form on the whole the most complete and concise account of the famous studies in conditioned reflexes that is available. The technology of the method, the general principles of interpretation of its results, and the results themselves in relation to many specific activities, are all set forth. The German translation is clear and very readable. No student working along the lines of Pawlow's methods should be without this exposition.

Self Consciousness and Its Treatment. By A. A. ROBACK. Cambridge, Massachusetts: Sci. Art Publishers. 1933. Pp. 122. \$1.50.

The publishers describe this as "The first scientific book on Self Consciousness." Actually it is a readable and lively study of the various drawbacks that may accompany an exaggerated preoccupation with one's own conduct and experiences. It contains many amusing illustrations and not a little advice. At its conclusion 100 questions are given which the earnest reader may endeavour to answer.

The Psychology of Laughter. By RALPH PIDDINGTON. London: Figurehead, 18 Adam Street, Adelphi. 1933. Pp. 227. 10s. 6d. net.

To Mr Piddington laughter is a form of social adaptation. It tends to arise whenever a situation occurs which has two different, and probably opposing, values attached, each of the values having a social basis. Laughter is a sort of true conservative way of dealing with this situation, for it means "After all, why worry? Everything is all right and nothing drastic need be done." This position is reached through a study of the biological function of elementary laughter, the relation of laughter and play, the psychological nature of weeping, the character and form of the ludicrous, and the nature of social sanctions. The argument is clear and persuasive, and as

solemn as are most arguments about laughter. Other theories are most excellently summarized and criticized, and both for this and for the author's own attempt to solve a difficult problem the book should get a wide reading. But what a pity that all analyses of the ludicrous seem bound to appear clumsy and portentous!

The Human Personality. By LOUIS BERG. London: Williams and Norgate, Ltd. 1933. Pp. xv + 321. 8s. 6d. net.

This is a book of the very popular type containing a large number of anecdotes and brief case studies and written in a dramatic style. Inevitably, perhaps, the author tends to make large numbers of sweeping and definite statements and to show little scientific caution. The general conclusion, that personality and character are an integration of many and diverse forces, is not one with which any psychologist will quarrel. The uncritical reader will doubtless find the volume very attractive, but he is likely also to reach the conclusion that psychological problems are much more simple and sharply defined than is in fact the case.

Introductory Sociology. By C. H. COOLBY, R. C. ANGELL and L. J. CARR. London: Charles Scribner's Sons. 1933. Pp. xii + 516. 15s. net.

As an introduction to sociology in the U.S.A., this book should be of the greatest use. For the English psychologist it contains less of interest. It deals firstly with the effects of heredity, primary grouping, external factors and culture upon human organization in general. Of more direct concern to the psychologist are the chapters on the Self, Emulation, and Conscience and Personal Degeneracy; but these embody a superficial and inadequate treatment of social psychology. The authors are then concerned to stress the importance of modern means of communication, their effect upon personality and upon social organization. The structure and functioning of the different groups in contemporary American society is then described clearly and thoroughly. The authors state that they have "tried to keep description and evaluation distinct"; but the latter, like cheerfulness, seems to 'keep creeping in,' giving a certain tendentiousness to an otherwise straightforward and comprehensive treatment of the subject.

The Individual and the Community. By W. R. LIAO. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1933. Pp. xv + 314. 15s. net.

This book is essentially a historical study. It describes and compares outstanding theories of the relation of the individual to the community, in the West from the time of the Greeks to Herbert Spencer; in the East from those of Confucius to recent days. A bibliography is included.

Crime, Law and Social Science. By J. MICHAEL and M. J. ADLER. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1933. Pp. xxix + 440. 15s.

In many ways this should be a useful book. Its four parts deal respectively with The Nature and Problems of Crime, Criminology, Criminal Justice, and Conclusions and Recommendations. A fairly good running and critical account is given of many researches in the causation, treatment, prevention and control of crime. The authors think that all the work done so far is of a very rough and unserviceable trial and error type. They also seem to be convinced both that psychologists have got to deal with the problems involved and that there are no psychologists who are capable of doing so. Their main practical recommendation is for the immediate establishment of a purely research 'Institute of Criminology.' They put forward an extremely detailed and definite plan of the organization of such an institute, and of the work which it ought to undertake. They would like to get a staff of psychologists and sociologists who combine all the virtues of the logician, the mathematician, the statistician, the

theoretical and the experimental physicist. They very justly consider that such a staff would be difficult to collect, so they suggest that the staff on the criminological side should consist of a logician, a mathematician, a statistician, a theoretical and an experimental physicist supplemented by "a mathematical economist and a scholar from the field of psychometrics" and a criminologist versed in the literature, but not hitherto a research worker. It is left to imagination how such a team would work together. The book, however, is dogmatic and stimulating.

A Survey of Mystical Symbolism. By M. A. EWER. London: S.P.C.K. 1933. Pp. 234. 8s. 6d. net.

This is an excellent brief study of the symbolism used consciously by mystical thinkers. It contains few interpretations, and is probably all the better for that. The author attempts to set forth the relationship between the main types of mystical symbolism so that "future students of mystical literature may see the position and relationships of the forms of expression they observe, . . . and the reader may perceive . . . the implications of that immense part of general literature which is tinged with mystical feeling. . . ." Mystical symbols are treated as attempts to find analogies between (a) sensory and spiritual experience; (b) physical and spiritual processes; (c) earthly and heavenly powers; and (d) earthly and spiritual relationships. Abundant illustrations are given. The book is exceedingly well written in a direct and lively style. A good selected bibliography is included.

History of Indian Philosophy. Vol. VII. By S. K. BELVALKAR and R. D. RANADE. Poona: Aryabushan Press Office, Shanwar Peth. 1933. Pp. 494. Rs. 15.

This volume will be of interest to many psychologists because it is devoted to a study of Indian Mysticism. An introduction deals with some of the general problems involved and makes some comparisons, not always clearly discussed, with Western mysticism. But the bulk of the volume is wholly expository and descriptive with, fortunately, very little attempt at criticism. As a source book for a psychological study of mystical experience the psychologist who is concerned with this difficult branch of investigation will find the work of great value.

A Bibliography of Aesthetics and of the Philosophy of the Fine Arts, from 1900 to 1932. Compiled and edited by WILLIAM A. HAMMOND. New York: Longmans, Green and Co., Ltd. 1933. Printed as a supplement to the *Philosophical Review*. Pp. viii + 183.

This Bibliography contains 2191 titles and an index. The research student who is concerned with this branch of investigation will doubtless find the book of value, though the indication of what is important and what is not might well have gone beyond the scrappy notes provided.

Consciousness: Brain-Child. By PERCY A. CAMPBELL. East Cleveland, Ohio. 1933. Pp. 109.

The Principles of Logic: An Introductory Survey. By C. A. MACE. London: Longmans, Green and Co., Ltd. 1933. Pp. xiii + 388. 12s. 6d.

Causality: A Law of Nature or a Maxim of the Naturalist? By L. SILBERSTEIN. London: Macmillan and Co., Ltd. 1933. Pp. viii + 159. 4s. 6d. net.

Idealistic Logic: A Study of its Method and Achievement. By C. R. MORRIS. London: Macmillan and Co., Ltd. Pp. vii + 338. 12s. 6d. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

- October 28, 1933. "The Rorschach Test applied to Children." By Miss MADELINE KERR, M.A.
"P-test Technique, and an Alternative Theory for *P*-factor." By WM. STEPHENSON, M.Sc., Ph.D.

SECTIONAL MEETINGS.

MEDICAL.

- October 25, 1933. "The Choice of Treatment." By Dr DENNIS CARROLL.

EDUCATION.

- October 2, 1933. "Psychology in the Training College." By Miss FLORENCE JOHNSON.
November 6, 1933. "Some Behaviour and Personality Difficulties in School Children." By Miss BERNICE McFIE.

INDUSTRIAL.

- October 12, 1933. "Correlation Methods in Psychology." By Dr J. O. IRWIN.
November 9, 1933. "On Being a Shop Assistant." By Miss M. A. LEIPER.

AESTHETICS.

- November 10, 1933. "Art—a Symbolic Language." By Mr MAXWELL ARMFIELD.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1933

Revenue Account for the year ended 30 September 1933

EXPENDITURE	30 Sept. 1932			30 Sept. 1932			INCOME		
	£	s.	d.	£	s.	d.	£	s.	d.
To Rent, 55 Russell Square	154	11	7	...	...	...	By Subscriptions 1933	...	...
Rent of Library prior to removal	35	0	0	...	...	...	Subscriptions 1930/1932	...	...
Secretary's Salary	175	0	0	...	...	...			
National Health and Unemployment Insurance	3	10	8	...	...	...			
Salary paid to former Secretary	100	0	0	...	...	...	Less Resignations and Removals	125	8
Printing	60	8	4	...	...	...			
Hire of Halls	32	2	0	...	...	...	Interest on Investments	...	...
Office and General Expenses:				...	...	...	Bonus on Assented War Loan	...	...
Caretaker, Lighting and Heating	31	3	5	...	...	...	Interest on Bank Deposit	...	...
Stationery	31	3	6	...	...	...	Interest on National Savings Certificates sold	...	...
Postage and Telephone	55	3	4	...	...	...			
Fire Insurance	4	10	3	...	...	...			
Subscriptions to outside Associations	6	16	6	...	...	...			
Audit Fee 1932	10	10	0	...	...	...			
Repairs	7	12	10	...	...	...			
Teas	11	13	6	...	...	...			
Sundry Expenses, Fares, etc.				149	8	10			
Journals Publication Expenses:									
General	168	13	3	...	...	...			
Medical	124	0	5	...	...	...			
Educational	94	3	4	...	...	...			
Scottish Branch Expenses	6	10	9	...	...	...			
Bank Charges	5	10	9	...	...	...			
Diploma of Honorary Membership	8	8	0	...	...	...			
Depreciation:				...	...	...			
Library	63	9	8	...	...	...			
Office Furniture and Fittings	27	0	5	...	...	...			
Legal Expenses re Lease, 55 Russell Square	37	0	0	...	...	...			
Balance, being Surplus of Income over Expenditure...	1254	2	6	...	...	...			
	135	15	4	...	...	...			
	£1389	17	10	...	...	...			
				£1213	13	1			
				£1389	17	10			
				£1213	13	1			

30 Sept. 1932			30 Sept. 1932			Assets		
£	s.	d.	£	s.	d.	£	s.	d.
LIABILITIES								
10	14	1	21	14	10	Cash at Barclays Bank Ltd.	...	...
35	0	3	5	8	5	Postage Stamps in hand	...	...
Sundry Creditors						Investments at Cost	...	...
Subscriptions in Advance						£100 New South Wales 5½ %	...	...
Publications Reserve						Loan 1924-34	...	...
72	0	0	...	...	...	£300 5 % Conversion Loan	100	0
Balance as per last Balance Sheet			...	...	...	1944-64	...	...
Revenue Account			...	...	...	£2645. 5s. 4d. 3½ % Inscribed	318	9
Balance as per last Balance Sheet			...	...	...	War Loan 1928-47	...	...
Add Surplus of Year			...	...	...	2717	6	9
4137	0	0	4329	18	8	3135 16 6		
						Note. Market value of above		
						Securities at 30 Sept. 1933	...	...
						£3129. 16s. 1d.	...	...
						141	13	0
						Subscriptions in Arrear	...	...
						Office Furniture and Fittings	...	...
						Balance as per last Balance Sheet	...	...
						243	5	0
						Add Expenditure during year	...	...
						16	12	0
						259	17	0
						Less Depreciation	...	...
						25	19	8
						243 5 0		
						Library		
						Balance as per last Balance Sheet	...	...
						571	6	6
						Add Expenditure during year	...	...
						44	19	5
						616	5	11
						Less Depreciation	...	...
						61	12	7
						571 6 6		
						Monograph Publications		
						Balance as per last Balance Sheet	...	...
						87	12	10
						Add Grant made during year	...	...
						50	0	0
						137	12	10
						Less Amounts repaid	...	...
						40	5	6
						87 12 10		
						Shares in the British Journal of Educational Psychology, Ltd., at Cost		
						50	0	0
						£4254 14 4	...	...
						£4407 19 8	...	...
						£4254 14 4	...	...
						£4407 19 8	...	...

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shown by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Banker's Certificate.

T. BARRETT,
Incorporated Accountant.
63 EASTCOTE LANE,
SOUTH HARROW.
6 November 1933.

S. J. F. PHILPOTT,
Hon. Treasurer.

ANNOUNCEMENT.

WE have received a notice from Professor Stern asking us to state that the journal now issued under the title of the *Zeitschrift für angewandte Psychologie* has no connection of any kind with the journal edited from 1908 to 1933 by Professor Stern and Professor Lipmann.

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THE BINOCULAR PERCEPTION OF FLICKER.

By M. D. VERNON.

(From the Cambridge Psychological Laboratory.)

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- II. *Method of experiment* (pp. 352-6).
 - (1) *Apparatus* (pp. 352-4).
 - (2) *Experimental procedure* (pp. 354-6).
 - (3) *Subjects of the experiments* (p. 356).
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- IV. *Effect upon monocular critical frequency of figured fields* (pp. 360-6).
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 - (2) *Figure in flickering field* (pp. 363-6).
- V. *Effect upon monocular and binocular critical frequency of binocular figured fields* (pp. 366-8).
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Appendix (pp. 373-4).
References (p. 374).

I. INTRODUCTION.

WHEN the retina is stimulated intermittently, so that the visual field is alternately lightened and darkened, the resulting visual impression is one of flicker. But if the rate of alternation is increased beyond a certain point, the flickering sensations fuse, and an impression of steady brightness results; the point at which flicker disappears is called the *critical frequency of flicker*. It has been shown by Porter (15) that the greater the intensity of stimulation, the greater must be the rate of alternation or flicker to produce a fused sensation. Much work has been carried out recently upon flicker, notably by Ives, Frank Allen, Granit, and Lythgoe and Tansley (references given subsequently). Using various wavelengths, intensities and retinal areas, they found that with constant

wave-length, retinal area and state of dark adaptation, the critical frequency is dependent upon brightness alone, according to the Ferry-Porter equation:

$$n = k \log I + k',$$

where n = number of alternations of light and dark, I = intensity of illumination, and k and k' are constants (14).

These observations were obtained with monocular stimulation and with plain visual fields. The work of certain other experimentalists, to be described later, seems to show that in binocular vision other factors than those mentioned may be of importance; and that in addition the nature of the visual field may itself exert some influence upon critical frequency. Consequently a series of experiments was undertaken to investigate these points. In the experiments about to be described binocular vision was used, although in most cases only one field of vision was flickering; and the brightness of the non-flickering field was varied, and also the nature and appearance of both fields. It will be shown that critical frequency is affected by these factors, and is not determined solely by the retinal and peripheral nervous factors mentioned above.

II. METHOD OF EXPERIMENT.

(1) *Apparatus.*

The apparatus consisted essentially of a large box, 33 in. long, 36 in. wide and 24 in. high, divided lengthways into two equal halves by a vertical partition. The subject of the experiment, by looking through the two prisms P_1 and P_2 (see Figs. 1 and 2) could observe the centres of the fields F_1 and F_2 provided by the back of the box, that of the right compartment with the right eye, and that of the left with the left. In each case an area subtending an angle of about 12° could be seen. The flickering or steady light was cast on these fields by two lamps L_1 and L_2 , shining through the apertures O_1 and O_2 of the light-tight boxes contained within the large box. The flicker was produced by rotating the discs D_1 and D_2 , each containing two alternate open and closed sectors of 45° , between the lamps and the apertures. The discs rotated on shafts connected by pulleys and belt drives to an intermediate pulley shaft, driven by a motor; they could be thrown in and out of gear (*i.e.* rotated or kept stationary) by means of suitable loose-running pulleys and braking arrangements. Thus both discs could be rotated simultaneously, at the same speed; or either could be rotated alone, the other being kept stationary.

The fields F_1 and F_2 were illuminated before the exposure by lamps

L_3 and L_4 ; the double triangular cardboard screens S_1 and S_2 , held in place by the electromagnets E_1 and E_2 , prevented the transmission of

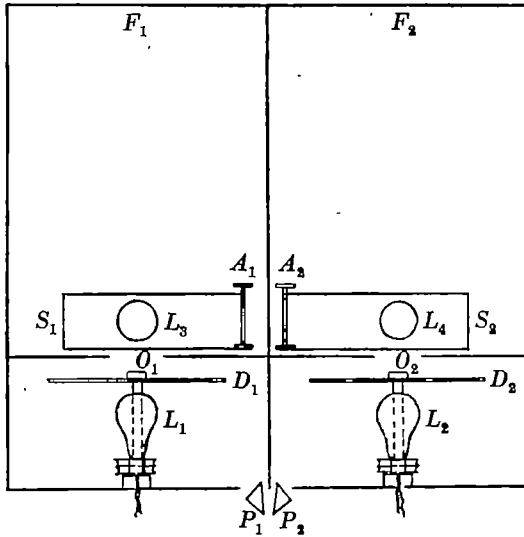


Fig. 1. Apparatus seen in horizontal section.

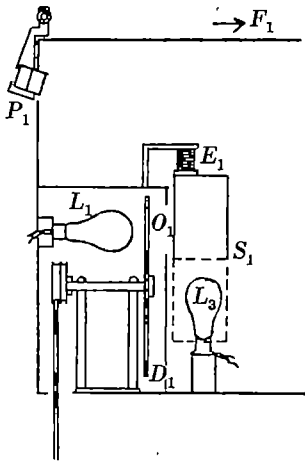


Fig. 2. Apparatus seen in vertical section.

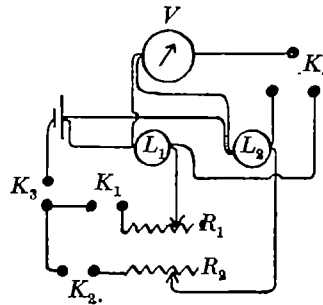


Fig. 3. Main lighting circuit.

light through the apertures O_1 and O_2 . When the current through the electromagnets was cut off, the screens dropped (by rotating about the axes A_1 and A_2 at their corners), cutting off the light from L_3 and L_4 , and allowing the light from O_1 and O_2 to fall on F_1 and F_2 .

The electric circuit through the lamps L_1 and L_2 is shown in Fig. 3. Either lamp could be switched on and off separately by the switches K_1 and K_2 , or both together by the switch K_3 . The illumination of the fields was varied in the earlier experiments by varying the brightness of the lamps by means of the rheostats R_1 and R_2 , the voltages being measured by the Everett Edgcumbe Voltmeter (type W.M.P.S.) at V , which could be connected to either lamp circuit by the two-way switch K_4 . In later observations (Series 5-7, see below), the brightness was varied by placing diaphragms of different diameters immediately in front of the apertures O_1 and O_2 . In all cases the illumination of the fields was calibrated directly with an Everett Edgcumbe Lumeter (recalibrated by the National Physical Laboratory). The brightness of the pre-exposure lamps, L_3 and L_4 , and also of the overhead lighting of the room (daylight was excluded), could be regulated by rheostats and a voltmeter in a separate circuit.

The speed of rotation of the discs was varied by means of a rheostat in the electric motor circuit, and was measured to the nearest 1 per cent. by means of a speedometer connected by a belt drive to the intermediate pulley shaft. The speed was frequently calibrated directly by means of a revolution counter.

(2) *Experimental procedure.*

In a preliminary series of experiments, an attempt was made to determine the threshold value for the critical frequency by gradually increasing the speed of flicker, or reducing the brightness, until the flicker disappeared, and then gradually bringing the flicker back again. It was found that the subjects, however, invariably had the greatest difficulty in determining the points at which flicker disappeared and reappeared. There was a large and variable region in which flicker was so slight as to be no more than a shimmer. Consequently the readings were so irregular that they were valueless. Moreover, the effect of variations in the time taken in making the judgments seemed to be appreciable; the critical frequency decreased rapidly as the time of stimulation increased. Granit and Von Ammon (8) found this effect to be considerable for the first 3 sec. of exposure in the periphery from 4° outwards. But in these experiments it seemed to be appreciable and progressive even at the centre. Consequently it was thought advisable to use the slower and more cumbersome method of limits, keeping all the conditions constant at each exposure, which lasted for 3 sec. in the earlier, 1 or 2 sec. in the later, series. The subject was required to say 'flicker' or 'no flicker,' basing his judgment upon whether he perceived any flicker at any time during the exposure.

Since with this method the time taken to obtain the threshold is much greater, it was unfortunately impossible to obtain sufficient results for statistical treatment, without completely exhausting the patience of the subjects. Whenever there was a consistent trend in one direction, however, they may be taken to be reliable.

Even with these precautions, considerable variations of critical frequency were found to occur both during a single sitting and also from day to day. During a sitting an average mean variation of as much as 4 per cent. was observed for one subject between the first and last readings, taken under otherwise comparable conditions, though the mean variation was under 1 per cent. for other subjects. But this variation took the form of a steady decrease of critical frequency throughout the experiment, presumably as the result of fatigue or some type of sensory adaptation to flicker. (It was not due to dark adaptation, since the stimuli were all fairly bright, and the room was well illuminated throughout.) Thus it was possible to eliminate the effects of this variation by a suitable order and arrangement of the presentations.

The day-to-day variation was irregular; the mean variation averaged about 1.5–2.5 per cent. for the various subjects. The effect of this variation was compensated for by reckoning each day's results as a percentage of some constant standard condition—usually the critical frequency with both fields plain, and the steady field either dark, or equal in brightness to the flickering field. Thus, in the results given below, all values are given as percentages. In the earlier series (1–4), the two eyes were stimulated alternately during each sitting; but in the later series it was found more convenient to alternate them only on different days.

In most of the experiments (Series 1–7)¹, the brightness of the fields was kept constant, and the threshold value obtained by varying the rate of rotation of the discs. Series 8 and 9 were designed in part, however, as a completion of some unpublished work by Dr D. N. Buchanan, in which the threshold was obtained by varying the brightness and keeping the rate of flicker constant. This method has numerous disadvantages, the chief one being that it is impossible with a lumeter to obtain completely accurate detailed calibration curves for the lamp voltages. Since the results throw some useful light on those of Series 1–7, they are, however, described below. It will be seen that the general trend of the results of these series is clear, even though small differences may not be altogether accurate.

The time of exposure of the flickering light was 3 sec. in Series 1–3;

¹ Details of the illumination and the nature of the fields are given in the Appendix.

but this time was then reduced, to avoid as far as possible the fatigue produced by the flickering light. In Series 4, 6 and 7, the figures were covered while the fields were illuminated by the pre-exposure lights, and only exposed at the moment at which the exposure lights were uncovered. This was done in order that the subject should only have a limited time for examining the figure, and hence would be less likely not to attend to it at all during the exposure. (It was intended that the subject should be as fully aware as possible of both the figure and the flicker throughout the exposure.) In Series 4, the time of exposure was cut down to 1 sec. (approximately), by the use of a pendulum carrying a screen which swung across the figured field, obscuring it after 1 sec. exposure. This device was cumbersome to use, however, and in Series 5-7 an exposure of 2 sec., timed by a stopwatch, was given. In Series 8 and 9 the subject was allowed to look at the figures before the exposure lights appeared—that is, during the pre-exposure period—in order to ensure stereoscopic fusion.

An artificial pupil was not used, since Lythgoe and Tansley (12) found that the variations in critical frequency produced by variations in pupil size were inconsiderable.

(3) *Subjects of the experiments.*

The subjects were all, except one, graduate students and research workers in psychology. The exception was a youth of seventeen employed in the laboratory. Since the apparatus did not interfere with the use of spectacles, those who normally wore spectacles were allowed to continue wearing them during the experiments. It was unfortunately not possible to have all the experiments performed by all the subjects.

III. THE CRITICAL FREQUENCY OF MONOCULAR AND BINOCULAR FLICKERING FIELDS.

The first object of these experiments was to determine the effect produced upon the monocular critical frequency of flicker by a steady non-flickering bright field exposed to the other eye. In Series 1 plain white flickering and steady fields were presented simultaneously to the two eyes, and the critical frequency measured for different illuminations of the steady field, the illumination of the flickering field being kept constant. In Table I and Fig. 4 (on the left) are shown the variations of the critical frequency for Subjects 1-4, expressed as percentages of the critical frequency when the steady field was dark. The values for the two eyes have been averaged together.

It appears that there was a gradual decrease in the critical frequency with increase of illumination of the steady field. For three subjects, the critical frequency was highest when the other eye was in darkness. The

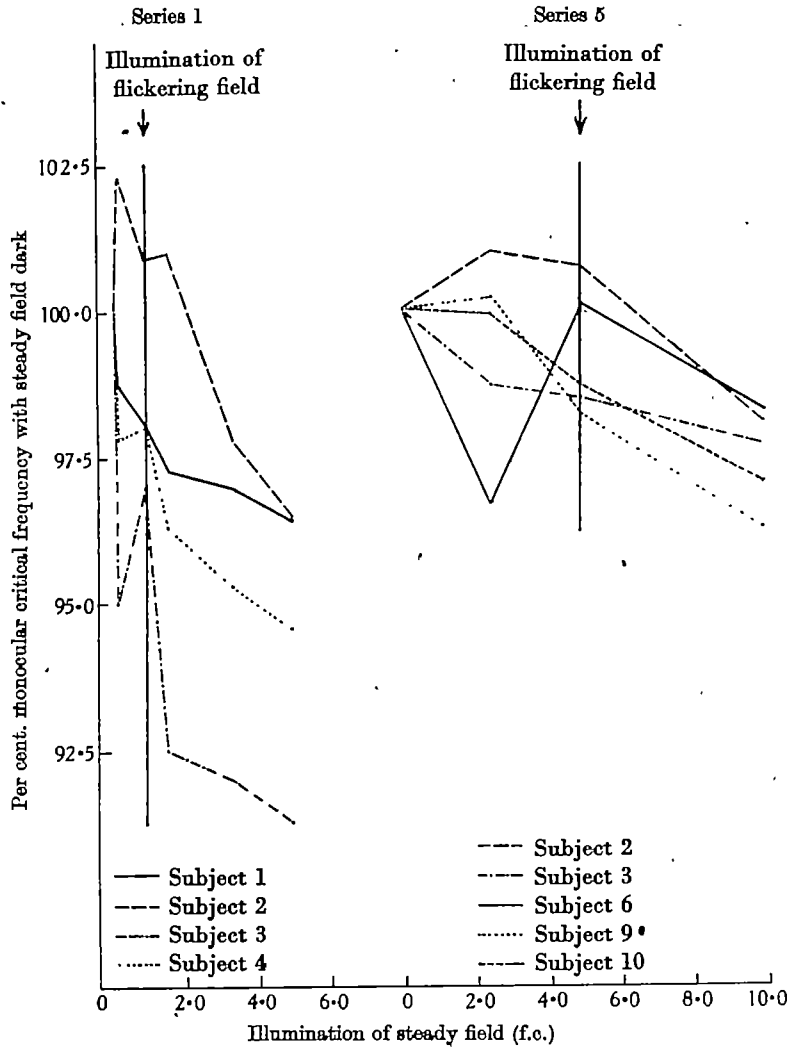


Fig. 4. Effect upon monocular critical frequency of varying the illumination of the steady field.

range of the decrease was from 3.6 to 8.7 per cent. for the different subjects, with an average value of 5.3 per cent. Since critical frequency decreases with decrease of intensity of the flickering light, we may say

that the phenomenal intensity at the eye illuminated by the flickering light decreased with increase of illumination of the other eye. It may be observed from Fig. 4 and Table I, however, that the decrease was much more rapid for illuminations of the steady field greater than that of the flickering field. For illuminations of the steady field less than that of the flickering field the effect is somewhat ambiguous. Indeed, for two of the

Table I. *Effect upon monocular critical frequency of variations in the illumination of the steady field (Series 1).*

Subject	Illumination of steady field, in f.c.s					
	4.90	3.32	1.48	0.85	0.12	0
1	96.4	97.0	97.3	98.1	98.7	100.0
2	96.5	97.8	101.0	100.9	102.3	100.0
3	91.3	92.0	92.5	97.0	95.0	100.0
4	94.6	95.3	96.3	98.0	97.8	100.0
Av.	94.7	95.5	96.8	98.5	98.5	100.0

four subjects the critical frequency, though higher when the steady field was in darkness than when it was equal in brightness to the flickering field, was also higher in the latter case than when the steady field was dimly illuminated. But the depressing effect of a really bright field upon the sensitivity of the other eye was much larger and more consistent.

Although the general trend of these curves is fairly obvious, there are numerous individual differences and inconsistencies. From the introspections of the subjects it was possible to deduce the existence of certain sources of error and difficulty in making flicker judgments. These were as follows:

(1) The rather long exposure time and the brightness of the white surroundings set up a glare which was sometimes dazzling and fatiguing to the eye.

(2) The size of the field was large enough to introduce considerable differences between the peripheral and foveal critical frequencies. Lythgoe and Tansley⁽¹²⁾ and Creed and Ruch⁽²⁾ have shown conclusively that with photopic vision the critical frequency is higher in peripheral than in foveal vision.

(3) The variation of the illumination of the field by varying the voltage of the lamps introduced colour variations of the field.

Consequently in Series 5-7 these factors were eliminated as far as possible by:

(1) reducing the exposure time to 2 sec. and making the surroundings black,

(2) reducing the fields to 5° of angle¹,

(3) varying the illumination of the field by placing diaphragms of different aperture in front of the lamps.

The range of illumination was also increased, to give a wider range to the observations.

Series 5 was designed to supplement Series 1, and determine the effect produced upon the monocular critical frequency of flicker by a steady, non-flickering field exposed to the opposite eye. The results are shown in Table II and Fig. 4 (on the right). They resemble those obtained in Series 1, but the effect produced by the steady light upon the critical frequency was much less; the range of the decrease was from 1.9 to 5.7 per cent. for the various subjects, and averaged 2.5 per cent. The depressing effect of the brightest steady light upon critical frequency was, however, unequivocal; though the amount was only small, it was quite consistent for all five subjects. But the effect of the less bright steady lights was again ambiguous. By comparing the two sets of curves in Fig. 4, it may be seen that it was only when the brightness of the steady field exceeded that of the flickering field that any marked falling off in critical frequency occurred.

Table II. *Effect upon monocular critical frequency of variations in the illumination of the steady field (Series 5).*

Subject	Illumination of steady field, in f.c.s			
	10.10	5.00	2.55	0
2	98.1	100.7	101.0	100.0
3	97.7	98.5	98.7	100.0
6	98.3	100.1	96.7	100.0
9	96.3	98.2	100.2	100.0
10	97.1	98.7	99.9	100.0
Av.	97.5	99.2	99.3	100.0

It is clear from these results that, except for Subject 2, no summation of brightness for the two eyes was discovered in these observations. In Series 8 and 9, however, there appeared to be a considerable increase in the critical frequency when both fields were flickering simultaneously². This result was therefore re-tested in Series 5. Both fields were flickering at the same rate (though not necessarily in the same phase), and at the same brightness, 5.0 f.c. The results are given in Table III, and show that

¹ It was impossible to reduce the field further without making the figures and diagrams in Series 6 and 7 too small to be clearly visible.

² Although for the sake of convenience these series are numbered after Series 6 and 7, they were actually presented before them.

the binocular critical frequency was greater, generally by about 5 per cent., than the monocular critical frequency, whether the steady field was dark, or illuminated at 5.0 f.c. That is to say, there was distinct summation of brightness of two flickering fields, but none with one flickering and the other steady. This curious result will be discussed again below.

Table III. *Comparison of monocular and binocular critical frequencies.*

Subject	Binocular flicker	Monocular flicker. Illumination of steady field in f.c.s	
		5.0	0
		96.6	95.9
2	100.0	96.8	98.2
3	100.0	95.2	95.1
6	100.0	93.5	95.2
9	100.0	94.2	95.4
10	100.0	95.3	96.0
Av.	100.0		

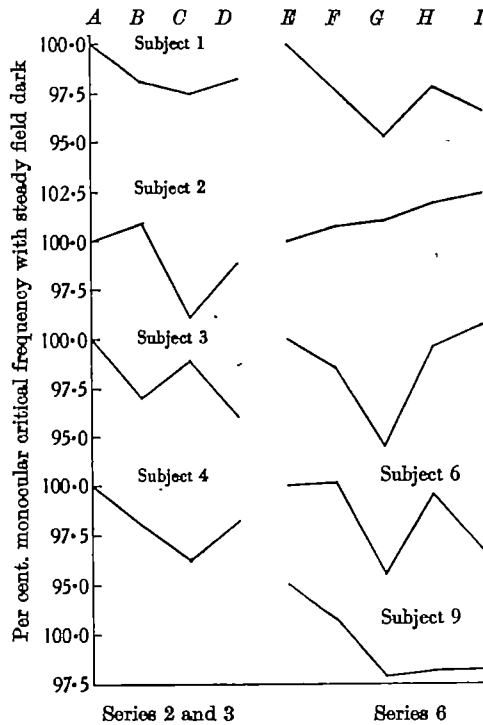
IV. EFFECT UPON MONOCULAR CRITICAL FREQUENCY OF FIGURED FIELDS.

(1) *Figure in steady field.*

The next step was to observe the effect upon the monocular critical frequency of introducing different types of figures into the flickering and steady fields. It seems from the results described in the last section that the less bright the flickering field relatively to the steady field, the less the former predominated over the latter in the fused binocular percept, and in consequence the less the awareness of flicker. It was thus thought that perhaps the same result might appear if other means were adopted of increasing the predominance of the monocular percept of the steady field over that of the flickering field, namely, by introducing figures of varying types into the former. To this end, figures graded in complexity were introduced into the fields in Series 2, 3 and 4: a dot in Series 2, an outline square in Series 3, and a variety of geometrical figures in Series 4.

In Table IV and Fig. 5 are shown the critical frequencies of the flickering field with an equally bright steady field, plain, containing a dot, or a square; these frequencies are expressed as percentages of the monocular frequency with the steady field dark. It is seen that there are slight indications of a reduction in the critical frequency, especially with a dot in the steady field. But the effect was not very clear or pronounced,

and in most cases was very little greater for the figured fields than for the plain field. In Table V are given the results of Series 4¹; the critical frequencies with figured steady fields are given as percentages of the critical frequencies with plain steady fields. Again the results are ambiguous. For Subjects 2, 3, 5 and 6 there were fairly large differences between the



Steady field: A, dark; B, plain; C, with dot; D, with square; E, dark; F, plain; G, with cross; H, with geometrical figure; I, with drawing of real object.

Fig. 5. Effect upon monocular critical frequency of presenting a figure in the steady field.

critical frequencies with the plain and figured steady fields; Subjects 2, 3 and 6 had a lower critical frequency with the figured steady field, Subject 5 a higher. It should be mentioned, however, that Subject 5 scarcely ever saw any flicker on or near the figure, but merely on one side, or towards one corner, of the field. It seems probable, therefore, that when the field was figured, the critical point was judged from peripheral

¹ In this series, through an error, the steady fields were rather brighter than the flickering field—1.48–0.87 f.c.

flicker vision only, whereas when the field was plain, the flicker was perceived centrally. This would account for the higher value of the critical frequency in the first case.

Table IV. *Effect upon monocular critical frequency of presenting a figure in the steady field (Series 2 and 3).*

Subject	Steady field dark	Steady field plain	Steady field containing	
			Dot	Square
1	100.0	98.1	97.5	98.2
2	100.0	100.9	96.1	98.9
3	100.0	97.0	98.9	96.0
4	100.0	98.0	93.8	98.1

Table V. *Effect upon monocular critical frequency of presenting a figure in the steady field (Series 4).*

Subject	Critical frequency of figured field as per cent. of that of plain field	Subject	Critical frequency of figured field as per cent. of that of plain field
1	97.9	5	103.8
2	94.3	6	95.8
3	96.9	7	101.1
4	101.4	8	102.2

This observation demonstrates a source of irregularity and variability which was mentioned previously—the variation of the critical frequency with the retinal area stimulated. Accordingly in the later series, 5, 6 and 7, a smaller area was stimulated; at $2\frac{1}{2}^{\circ}$ from the centre the critical frequency is not appreciably greater than that at the centre. In Table VI are given the results for Series 6 and 7, and in Fig. 5 they are compared with those of Series 1, 2 and 3. We see that the critical frequency was appreciably decreased for four subjects when there was a

Table VI. *Effect upon monocular critical frequency of presenting a figure in the steady field (Series 6 and 7).*

Subject	Steady field dark	Steady field plain	Steady field containing		
			Cross	Geometrical figure	Drawing of real object
1	100.0	—	96.2	98.6	96.6
2	100.0	100.7	101.0	101.9	102.4
3	100.0	98.5	94.5	99.6	100.7
6	100.0	100.1	95.5	99.5	96.7
9	100.0	98.2	95.4	95.6	95.7

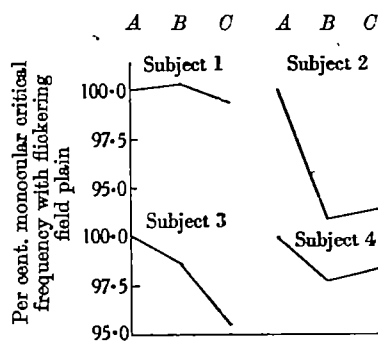
cross in the steady field, for three subjects when there was a drawing of a real object, but only for one subject when there was a geometrical drawing. The last result seems a little inexplicable. The subjects were required to reproduce these geometrical figures afterwards, in order to ensure that they did not ignore it altogether and concentrate only on the flicker. The figures were rather complicated, and the subjects reported that they tended to forget whether or not the field was flickering; and one can only surmise that, in the effort to remember the figures, the flicker judgments were given at random, and hence were too variable to show any consistent trend. The results for the cross and the drawing of the real object are clearer. But it does not appear that the effect produced by the latter was any greater than that produced by the former. Consequently this effect seems to have been independent of (a) the amount of patterning or figure in the field, (b) the interest and attention aroused by the figure (which was usually considerable with the animal drawings). Indeed, if anything, the very simplest figures, the dot in Series 2 and the cross in Series 6, seemed to produce the greatest effect upon critical frequency. Judging from their introspections, the subjects usually devoted a shorter period of time out of the total exposure to making their flicker judgments when there were figures in the field. But this was not necessarily due to any interest in the figures; rather, the latter forced themselves into the centre of awareness, and made it impossible to give reliable and certain flicker judgments without a considerable effort of will. But this phenomenon was reported for the simpler, as well as for the more complex figures.

Some of the discrepancies in these results may have been due to the fact, also reported by the subjects, that it was sometimes hard to keep the plain unfigured fields in focus. Thus the flicker was sometimes not observed when it approached the critical frequency. This would tend to lower the critical frequency with the steady field plain relatively to the critical frequency with the steady field figured.

(2) *Figure in flickering field.*

It is now interesting to observe if any effect is produced upon critical frequency by introducing a figure into the flickering field, and compare that effect, if any, with that produced by a figure in the steady field. From Table VII and Fig. 6 it appears that in Series 2 and 3, with purely monocular stimulation, the steady field being kept dark, the critical frequency was on the whole reduced by introducing a dot or a square into the flickering field; the effect was, in general, greater than that produced

by a figured steady field, and was consistent, except for Subject 1. It seems curious that so small and insignificant a figure as a dot should produce so considerable an effect; again it cannot be ascribed to a darkening of the field by the pattern, or to a concentration of interest and attention on the figure. It is possible, however, that the dot tended to



Flickering field: A, plain; B, with dot; C, with square.

Fig. 6. Effect upon monocular critical frequency of presenting a figure in the flickering field.

concentrate vision upon the centre of the field, thus eliminating any peripheral perception of flicker, which might have occurred with a plain field.

When, however, in Series 4, a geometrical figure was introduced into the flickering field, with the steady field plain and rather brighter than

Table VII. *Effect on monocular critical frequency of presenting a figure in the flickering field (Series 2 and 3).*

Subject	Flickering field		
	Plain	With dot	With square
1	100.0	100.3	99.4
2	100.0	93.4	93.9
3	100.0	98.6	95.5
4	100.0	97.8	98.4

the flickering field, it was found that in general the critical frequency was raised (see Table VIII). The effect was slight, but was shown by six subjects out of eight. In no case was there any appreciable reduction of critical frequency.

It was hoped that the results of Series 7 would throw some light upon these contradictory results. In this series, as in Series 5 and 6, the area of the stimulus was only 5° of angle; the figures were similar to those of

Table VIII. *Effect on monocular critical frequency of presenting a figure in the flickering field (Series 4).*

Subject	Critical frequency of figured field as per cent. of that of plain field	Subject	Critical frequency of figured field as per cent. of that of plain field
1	105.9	5	102.2
2	101.0	6	103.0
3	101.2	7	102.4
4	99.1	8	99.9

Series 6, but appeared in the flickering field. Unfortunately, as will be seen from Table IX, these results are themselves contradictory. Subjects 2 and 9 showed an increase of critical frequency when figures were introduced into the flickering field, the steady field being plain and of equal brightness. Subjects 3 and 6 showed a decrease.

Table IX. *Effect on monocular critical frequency of presenting a figure in the flickering field (Series 7).*

Subject	Flickering field plain	Flickering field containing		
		Cross	Geometrical figure	Drawing of real object
1	100.0	100.1	101.6	99.6
2	100.0	102.2	102.6	101.4
3	100.0	98.8	98.7	98.3
6	100.0	100.9	99.3	96.5
9	100.0	100.3	104.7	103.3

A possible explanation of this effect seems to be that the inhibitory effect of the steady white field was in some cases greater when the flickering field was plain than when it was figured. Thus in Series 4, and in some cases in Series 7, this effect was sufficiently great to swamp the inhibitory effect produced by the introduction of figures into the flickering field. A large inhibitory effect was produced by interaction of the plain steady field with the plain flickering field; and this effect was actually reduced by the simultaneous presence of figures in the flickering field. This supposition is the more probable in that, as mentioned previously, the steady field was inadvertently made brighter than the flickering field in Series 4; and, as may be seen from Fig. 4, critical frequency was considerably lower when the steady field was 1.45 f.c. than when it was 0.87 f.c. Thus

we may suppose that a steady field considerably brighter than the flickering field produces a greater effect upon the latter when plain than when figured.

V. EFFECT UPON MONOCULAR AND BINOCULAR CRITICAL FREQUENCY
OF BINOCULAR FIGURED FIELDS.

(1) *Monocular flicker.*

If a decrease in critical frequency is produced by the introduction of a figure into the steady field, and is also produced by the introduction of a figure into the flickering field, it might be expected that a greater decrease would be produced by introducing figures into both fields. Such figures must of course be quite or almost identical to ensure fusion and eliminate rivalry. In Series 8 and 9 the simple cross figures were identical; the more complicated line drawings of geometrical solids and shaded drawings of real objects were stereoscopic. Most of the subjects reported, however, that the effect of relief was not great, and only on one or two occasions did the flicker and figure appear in different planes. Consequently the effects seem to resemble on the whole those produced by two identical fused monocular figures.

In Table X and Fig. 7 are given the results obtained when both the fields were figured, but only one flickering. As noted above, the illumination of the fields was varied in these series, and the rate of flicker kept constant. Since the rate of flicker varies with the logarithm of the intensity of illumination, these results have been plotted on a logarithmic scale, to make them comparable with those of the earlier series. When this is done, the increase in the threshold (increase of brightness, or decrease of critical frequency) still appears to have been large and consistent, considerably more so than when only one field was figured. We

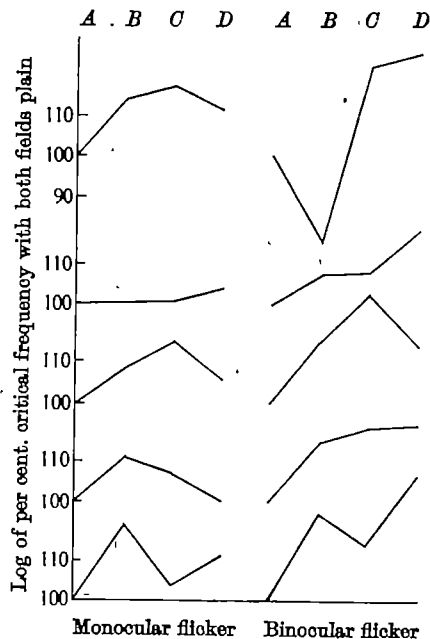
Table X. *Effect on monocular critical frequency of presenting figures in both fields simultaneously.*

Subject	Fields plain	Fields containing		
		Crosses	Stereoscopic drawings of	
			Geometrical solids	Real objects
2	100.0	114.1	117.5	111.5
6	100.0	—	100.6	103.9
7	100.0	108.5	115.1	105.3
8	100.0	110.7	107.0	100.0
9	100.0	119.2	103.6	111.0

must conclude, then, that some increase in the threshold is produced by introducing a figure into the flickering field, and that this effect summates with that produced by introducing a figure into the steady field to give an even greater effect when both fields are figured. Again, this effect is on the whole greater with the simpler figures, the cross and the geometrical solids.

(2) *Binocular flicker.*

In Series 9 both fields were made to flicker simultaneously, at equal rates and brightnesses. The results are shown in Table XI and Fig. 7.



Fields: A, plain; B, with cross; C, with solid; D, with real object.

Fig. 7. Effect upon critical frequency of presenting figures in both fields simultaneously.

In the first place, a general lowering of the threshold took place, as was noted above in Section III of this paper. But superimposed upon this a consistently larger increase in the threshold than in Series 8 was produced by introducing figures into the fields. That is to say, the depressing effect of the figures was greater when both fields were flickering than when only one was flickering. This seems to show that the effect of the figure is greater when actually present in the flickering field than when in the

steady field (always provided its effect is not complicated by the presence of a bright, plain, steady field). This confirms the results obtained in Series 2 and 3 (see p. 363). But this effect was not great enough to raise

Table XI. *Effect on binocular critical frequency of presenting figures in both fields simultaneously.*

Subject	Fields plain	Fields containing		
		Crosses	Stereoscopic drawings of	
			Geometrical solids	Real objects
2	100.0	81.4	122.8	131.0
6	100.0	107.2	107.6	118.5
7	100.0	115.0	129.1	114.5
8	100.0	117.5	118.6	119.8
9	100.0	122.8	114.5	134.1

the threshold to the level which obtained when only one field was flickering. The summative effect of the binocular flicker was still more powerful than the inhibitory or depressing effect of the binocular figures.

Thus the following conclusions have been reached:

Effect of plain, bright, steady field > effect of dark field.

Effect of figure in steady field > effect of plain, bright, steady field.

Effect of figure in flickering field > or < effect of plain, bright, steady field.

Effect of figure in flickering field > effect of figure in steady field.

Effect of figure in both fields (one flickering) > effect of figure in one field only.

Effect of figure in both fields (both flickering) > effect of figure in both fields (one flickering).

Summative effect of binocular flicker > inhibitory effect of figures.

In fine, the greater the number of presentations we add to the binocular percept, the greater the decrease in visual sensitivity as measured by the critical frequency of flicker. Yet we have seen that this conflict is not simply one of interest and attention, for the decrease is produced as much by the simple and familiar figures as by more complicated and novel ones. Moreover, it should be noted that if both fields are flickering simultaneously at the same rate, the conflict is replaced by a confluence or summation.

VI. DISCUSSION.

That these results seem rather confusing and conflicting is hardly surprising in view of the contradictory results obtained by other investigators with regard to binocular interaction and summation of brightness. Thus Sherrington⁽¹⁷⁾ found that the binocular interaction between two foveal flickering fields was very slight indeed, although some summation did occur when the phases of the flicker were synchronous for the two eyes. But the effect of a monocular field of steady brightness was to decrease the critical frequency of the other, flickering field, whether the former was brighter or dimmer than the latter. Actually the reduction produced by a brighter steady field seems to have been rather greater than that produced by a darker steady field (9 to 4 per cent.). These results were controverted by De Silva and Bartley⁽⁵⁾, who found that, with steady non-flickering lights, the dimming of one field always produced dimming of the binocular field. This dimming was greater with dim stimuli than with bright ones, and with a larger field than with a smaller, foveal one. The binocular dimming was relatively less than the actual physical monocular dimming with small degrees of dimming, but greater than the actual monocular dimming for high degrees of dimming. This agrees with the phenomenon discovered by Fechner⁽⁶⁾ and known as Fechner's paradox; though Fechner placed the maximum subtractive effect with intensity ratios of 1:25, while De Silva and Bartley put it at 1:8 or 1:15. These results are also in agreement with those of Frank Allen⁽¹⁾, who found that dark adaptation of one eye produced a decrease in the critical frequency of flicker of the other eye; while prolonged stimulation of one eye with bright light produced an increase of critical frequency of the other eye. G. Hartmann⁽⁹⁾ found that a corresponding increase in visual acuity, as measured by the smallest perceptible separation of two black squares on a white ground, white squares on a black ground, or grey squares on a grey ground, was produced by presenting a very bright light to the opposite eye 2 sec. before the measurement of acuity began.

Zigler and Ward⁽¹⁸⁾ found that a shift from binocular to monocular vision produced a darkening of the field in 90 per cent. of presentations of plain, coloured and figured fields subtending an angle of 4° at the eye. But Laird⁽¹¹⁾ found that binocular summation of brightness occurred only in scotopic vision; in photopic vision the brightness of the binocular sensation approximated to that of the monocular sensation of the keener eye. And a number of observers, including McDougall⁽¹³⁾ and Piper⁽¹⁴⁾,

found that binocular summation of brightness occurred only with very dim stimuli; the latter explained this alteration in brightness as a function of dark adaptation.

One can only conclude from these diverse results that any effect of summation is less pronounced, if not absent, with bright stimuli and foveal stimulation; and that the summative effect is greater with peripheral vision and with dim stimuli or dark adaptation. It has been shown by Granit(7), Creed and Ruch(8) and others that spatial summation of brightness is considerable in the periphery of the single retina, but this effect is usually ascribed to synaptic convergence, and cannot for that reason bear any relation to binocular summation. Even should the size of the areas account for the summative effects of binocular flicker demonstrated in these experiments, why should flicker summate, and not steady brightness and flicker, though flicker (at a given rate) is said to depend wholly upon brightness? It is difficult to account for the depression of frequency produced in these experiments by the steady bright areas, even if this is a purely foveal effect similar to that demonstrated by Sherrington. In fact, it is difficult to give any explanation of these phenomena except by some species of interaction at the central nervous level.

Since the results obtained in the work of these other investigators are so conflicting, and seem to give so little assistance in arriving at a possible explanation for the results obtained in the experiments described in this paper, the writer is inclined to advance a purely phenomenological explanation. This is foreshadowed in some observations made by Shepherd Dawson(4), and by Zigler and Ward(18). Thus Dawson found that the monocular and binocular percepts evoked by the same stimulus, a grey ring on a slightly lighter grey ground, were very different in character. The monocular percept was more intermittent than the binocular; was sometimes elusive and fluctuating, or hazy, filmy and ghost-like. It was seen and located less easily, and its edges were less distinct. Though the smallest detectable difference in brightness was the same for monocular and binocular vision, the monocular observations were less certain and steady, and more difficult to make. Zigler and Ward found that the transition from binocular to monocular vision was attended, not only by a decrease in brightness, but also by a loss of saturation of colour, a decrease in definiteness, clearness and 'hard surface characteristics,' a tendency to lack 'objective reference,' and a tendency of the surface to soften and develop a 'thin, cloud-like effect.'

Now it was characteristic of the observations described in this paper that there was a considerable range round about the threshold when the

flicker was shimmering, hazy, indefinite, and when it was very difficult to say if there was flicker or not. The suggestion is hazarded that when flicker is binocular there is an increase in clearness and definiteness in the appearance of flicker, which would fix the threshold in the lower regions of this range. With monocular flicker, however, the percept of the bright, steady field would tend to 'dilute' the flicker percept, and make it even more hazy than when it is purely monocular, *i.e.* when the steady field is dark; hence the threshold would be reported to lie at the upper end of the range of indefiniteness. This effect would tend to increase with increasing brightness of the steady field. Moreover, with large areas of stimulation and bright surroundings, as in Series 1, it might well be greater than with smaller areas and dark surroundings, as in Series 5; and in fact the depressing effect was greater in Series 1 than in Series 5.

The presence of figures in the field would have somewhat the same effect. To borrow the Titchenerian phraseology used by Dallenbach (3), they would render the flicker 'attributively unclear' by their own 'attributive clearness,' even when they lacked any cognitive attentive value or interest value. Their clear, dark lines would be focused at the centre of awareness, making purely marginal the apprehension of the indefinite, hazy flicker phenomenon. This would account for the effect produced by figures both in the flickering and in the steady fields. The existence of this conflict is confirmed to some extent by the subjects' introspections which were quoted above. They also reported sometimes that if flicker was particularly clear and vivid, it interfered with perception of the figures, which were ignored or forgotten in consequence; *i.e.* the attributive clearness of the flicker was greater than that of the figure, hence the former occupied the centre of awareness. On the occasions on which the critical frequency seemed to be lower when the steady field was plain and the flickering field figured than when both were plain, it is to be supposed that the plain field 'diluted' the effect of the figure, as well as that of the flicker. Hence the effect of the figure was no greater than, or even less than, that of the plain field. On the other hand, the conflict between the figure and flicker seems to have been more acute when both flicker and figure were in the same field, and the steady field was dark, than when one was in one field and the other in the other—that is to say, the effect upon critical frequency was greater. It seems understandable that a direct monocular conflict of clearness might be greater than a transferred binocular conflict.

The binocular figures again would tend to produce a greater effect upon the threshold than the monocular ones, since they would themselves be clearer and more definite—as noted by Dawson, and by Zigler and

Ward—than the monocular ones. This effect presumably was greater with binocular flicker than with monocular flicker, since a greater effect is produced by a figure in the flickering field than by one in the other, steady, field.

This explanation may seem somewhat fanciful in accounting for what is usually held to be an unequivocal example of the 'constancy hypothesis,' a phenomenon regulated and determined on the purely sensational level, if not in the retina and peripheral nervous system. Yet these lower level processes seem quite inadequate to account for the phenomena of binocular interaction which have been described above. On the other hand, we have seen that the 'higher' cognitive processes such as interest have little if any bearing on the results. Consequently it seems reasonable to postulate that these phenomena are regulated in accordance with some intermediate process which we may call 'central and marginal attributive clearness of awareness.' Or, to put it in another terminology, the figures and the flicker provide a demonstration of Rubin's 'figure and ground' phenomenon (16). In general the figures of these experiments constituted the 'figure,' and the flicker the 'ground'; but occasionally the flicker spontaneously assumed the rôle of 'figure,' and more often it was forced into that part by a voluntary effort of the subject for the purpose of making his flicker judgment. The relative dominance of the 'figure' over the 'ground' in the binocular percept was determined by the nature of the conflicting monocular percepts, according to the scheme drawn up on p. 368. This hypothesis is in agreement with the results obtained by L. Hartmann (10). He found that the flicker disappeared more easily (*i.e.* at a lower frequency) in a field that was perceived as 'ground' than in one that appeared as 'figure.' And we must conclude that the flicker threshold is a phenomenon which is determined purely by rate and brightness only when it occurs in a plain, unfigured monocular field; with more complex fields it is subject to a number of variations which are not governed by purely quantitative laws.

VII. SUMMARY.

The critical frequency of flicker in the monocular perception of a plain white field was found to be modified in binocular perception. It was shown that monocular critical frequency:

(1) Was reduced when the steady field presented to the other eye (a) was brighter than the flickering field, (b) contained a black figure of varying complexity.

(2) Was reduced when a black figure was presented in the flickering field; but this effect was masked by a presentation of a plain, steady field to the other eye.

(3) Was increased when both fields were made to flicker at the same rate.

(4) Was reduced considerably when figures were presented in both fields, especially when both fields were flickering.

An attempt has been made to account for these phenomena in terms of (a) a conflict in the binocular percept between unlike monocular percepts, and a reinforcement between like ones, (b) a conflict in awareness between the different features of the binocular percept, *i.e.* the figures and the flicker.

In conclusion, my acknowledgments are due to the Medical Research Council, a grant from which has made this research possible; and my thanks to Prof. Bartlett for permission to work in his laboratory, and for his advice and assistance.

APPENDIX.

Illumination and Nature of the Fields.

Series 1-4, and 8 and 9. Size of fields 12° approximately; surroundings white.

Series 5-7. Size of fields 5° ; surroundings black.

Series 1. Illumination of flickering field, 0.87 f.c.; of steady field, 0, 0.12, 0.85, 1.48, 3.32, or 4.90 f.c. Both fields plain white; illuminated by flickering and steady light alternately.

Series 2. Illumination of flickering field, 0.87 f.c.; of steady field, 0.85 f.c. One field plain white, the other with a black dot in the centre; illuminated by flickering and steady light alternately.

Series 3. Illumination of both fields as in Series 2. One field plain white, the other with a large black outline square in it; illuminated by steady and flickering light alternately.

Series 4. Illumination of flickering field, 0.87 f.c.; of steady field, 1.48 f.c. One field plain white, the other containing a large black outline geometrical figure in it. This figure changed at each exposure, though repeated later in the series. Illuminated by steady and flickering light alternately. Also controls with both fields plain.

Series 5. Illumination of flickering field, 5.0 f.c.; of steady field, 10.1, 5.0, 2.55, or 0 f.c. Both fields plain white; illuminated by steady and flickering light alternately. Observations also made with both fields flickering.

Series 6. Illumination of flickering and steady fields, 5.0 f.c., except for observations on monocular flicker, when steady field was dark. Figures of varying complexity exposed in steady field only. These were (1) a black cross, (2) a series of black geometrical figures (different at each exposure), (3) a series of black line drawings of animals (different at each exposure).

Series 7. Illumination of steady and flickering fields, 5.0 f.o. Figures of varying complexity exposed in flickering field; (1) and (2) as in Series 6, (3) a series of line drawings of domestic objects. Also controls with both fields plain.

Series 8. One field flickering at speed of 1200 revs. per min., the illumination being varied; the other steady, at an illumination of 1.48 f.o. Fields alternated. Two like figures of varying complexity presented simultaneously in both fields. These were (1) black crosses, (2) black line stereoscopic drawings of geometrical solids, (3) shaded black stereoscopic drawings of real objects. Also controls with both fields plain.

Series 9. Both fields flickering simultaneously at same speed, 1200 revs. per min. Illumination of both fields varied equally. Figures similar to those in Series 8.

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(Manuscript received, 2 September, 1933.)

AN EXPERIMENTAL STUDY OF THE CONTROL
OF THE MECHANISM OF SPEECH, AND IN
PARTICULAR OF THAT OF RESPIRATION,
IN STUTTERING SUBJECTS.

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University of Edinburgh.)

- I. *Introductory* (pp. 375-6).
- II. *Respiration during speech in stutters and normal speakers*
(pp. 376-9).
- III. *Experimental study of the control of the speech-mechanism in
stutters and normal subjects* (pp. 379-87).
 - (a) *The problem* (pp. 379-80).
 - (b) *Experimental procedure* (pp. 380-3).
 - (c) *Results and conclusions* (pp. 383-7).
- IV. *Summary* (pp. 387-8).
References (p. 388).

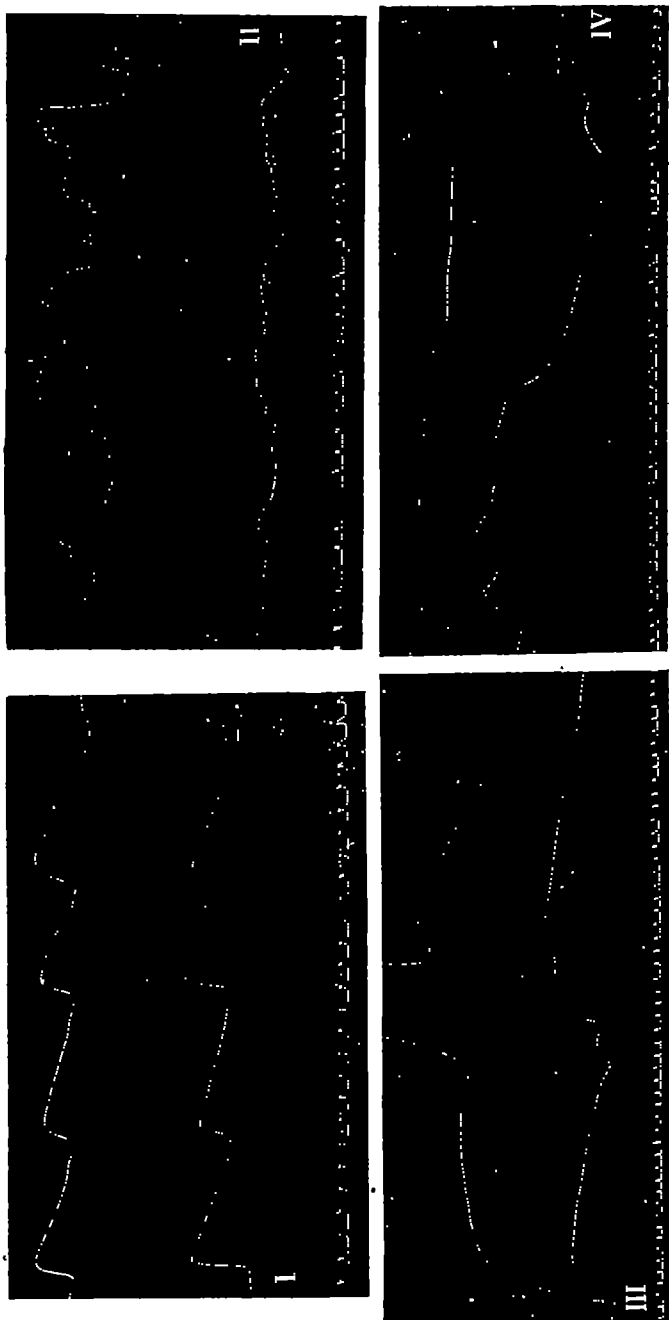
I. INTRODUCTORY.

THE history of the theory and methods of treatment of stuttering which prevailed even during the nineteenth century (1) is a record of imperfectly realized guesses at truth and misguided attempts at cure by operative methods which showed no advance in understanding upon the cauterization practised by Galen, or the instruction in elocution and exercises in articulation after the manner of those which were recommended by Mercurialis in the sixteenth century. Nearly all the students of the condition misconceived it as a *defect* of speech, to be considered along with and treated analogously to other defects, such as interdental lisping or 'burring,' instead of distinguishing stuttering clearly from these as a *disorder* of speech in which the mechanism of utterance was in nearly every case intact, and the capacity to speak satisfactorily under certain conditions unimpaired. But if we disregard the crudest of the anatomical theories we find that some investigators of the problem had looked sufficiently beyond the peripheral organs of speech and the most

obvious symptoms of the disorder to be able to trace the stuttering to the disturbance of breathing which was apparent during the stutterer's attempts to speak. Coën went so far as to argue that the stutterer's faulty speech was the result of a general weakness and irregularity of his respiration, and put forward the opinion that the pressure of the pulmonary air was ordinarily lower in the case of the stutterer than in that of the normal speaker. The belief that respiratory anomalies extending beyond the actual period of stuttering are present as the ground of the disturbance of speech has persisted with more or less modification up to the present time. Even now it finds influential support in the work of Gutzmann's Berlin school(4). But the theory raises important questions which still await a definite answer. On the one hand, are the manifestations of faulty respiration which are readily observable during periods of stuttering speech confined to those periods? Are they, that is to say, simply a less obvious and perhaps secondarily developed symptom of the disorder without fundamental significance? Or, on the other hand, are the inefficient habits of breathing present also under conditions which do not give rise to stuttering or, still more significantly, in circumstances in which the stutterer is not required to speak? Answers to these questions have been given in directly opposite senses by Gutzmann himself, who maintains that there are anomalies of breath-control which are characteristic of the stutterer and are present independently of the demand for speech, and by Fletcher(3), who finds no characteristics of breathing peculiar to the stutterer apart from those which occur during stuttering speech.

II. RESPIRATION DURING SPEECH IN STUTTERERS AND NORMAL SPEAKERS.

As this paper is not primarily concerned with the stutterer's speech performances, no detailed analysis will be attempted of the bewildering manifestations of disorganization under these conditions which are apparent in his breathing. A number of kymograph records are presented, however, which serve to illustrate the most striking of the divergences from the normal which occur in the breathing of the stuttering subject during the reading aloud of a passage of continuous prose. In the simultaneous recording of the abdominal and thoracic breathing curves, two belt-pneumographs of the type described by Gutzmann were employed. These pneumographs are constructed on a base formed by a stout rubber belt, to the inner side of which a strip of finer rubber is attached to form in connection with the recording-tambour the usual closed air-system.



Figs. I-IV. Upper curve—Thoracic breathing. Middle curve—Abdominal breathing. Lower curve—Time in $\frac{1}{4}$ sec.

During normal speech by a non-stuttering subject the respiration curves show a regular and characteristic form (Fig. I), of which the distinguishing features are:

- (1) the rhythmic relation between inspiration and expiration;
- (2) the marked prolongation of the period of expiration by comparison with ordinary rest-breathing;
- (3) short inspiration followed by a period of smooth expiration regulated in accordance with the logical sequence of expression;
- (4) very close synchronization between abdominal and thoracic breathing, thorax and abdomen functioning in strict co-ordination.

The three records (Figs. II, III and IV) of breathing during stuttering were all obtained while the subjects were attempting to read aloud the same passage of prose as was read by the normal speaker whose record is shown in Fig. I. The records of the stuttering subjects reveal abnormalities in all four of the principal characteristics of breathing during speech which have been enumerated above. In all of the records the typical relationships between expiration and inspiration have disappeared, and the whole process has become arrhythmical in the extreme. Speech and the thought to be expressed, and further back and all the more distinctly, respiration and thought, are out of tune. Fig. II, a record taken during more or less continuous speech, in which the principal overt manifestation of the disorder consisted in clonic repetitions which never became severe, illustrates very clearly this annihilation of the normal rhythm. Instead of a regular sequence of synchronized rise and fall in the curves, the record shows a highly irregular progress in which inspiration is interrupted by expiration, or expiration by inspiration, apparently at haphazard, and where sudden involuntary releases of the air in the lungs, especially marked in the thoracic curve, have to be compensated for by a prolonged inspiration, which is contrary to and continually interrupted by the attempt at utterance. The second stuttering record, Fig. III, shows even more clearly the disappearance during stuttering reading of the temporal sequence and the thoracic-abdominal phase relationships of normal respiration. When this record was produced the subject exhibited all the signs of very severe distress during reading. The record reveals severe tonic cramp in the mechanism of respiration, followed by a strong inspiratory spasm of the thorax which is not paralleled at all in the abdominal curve. Similar remarkable fluctuations of the thoracic curve were several times repeated in a single record. Fig. IV is a record of breathing during *attempted* speech, since at no time during the period of this tracing, which occupied approximately 20 sec., was the subject able

to articulate distinctly a single word. For the greater part of the time he was quite silent, never achieving more than a momentary laryngeal murmur.

Considered together, these records of breathing during stuttering speech reveal, by comparison with the normal:

(1) a complete disorganization of the normal rhythm of respiration during speech;

(2) apparently unpremeditated, although not always involuntary, halts, interruptions, sudden releases, and complete reversals of the process of expiration or inspiration;

(3) a breakdown in the more or less strict parallel phase relationship which normally exists between the movements of the thorax and the abdomen during speech.

III. EXPERIMENTAL STUDY OF THE CONTROL OF THE SPEECH-MECHANISM IN STUTTERERS AND NORMAL SUBJECTS.

(a) *The problem.*

Disturbance of breathing during the period of utterance forms part of the failure of the co-ordinated functioning of the speech-mechanism which distinguishes stuttering speech. Whether this disturbance is merely a more pronounced expression of an abnormality of the respiratory process which is present under any and all conditions is very doubtful. But even if this cannot be proved, it is still possible that the stuttering subject has less than the normal degree of *control* over the mechanism of speech even when speech is not required of him. If that were so, it should not be difficult to trace anomalies in his performance when he attempts voluntarily to regulate the movements of that mechanism for purposes other than speech.

It has been argued by Fletcher (3) that the disorder of stuttering takes its rise in and is maintained by psychological factors which become effective only in connection with the willed act of speech, and then only when speech is used for purposes of communication. If any one of the elements in that activity be altered, all trace of the disturbance should disappear. Allow the stutterer to read in unison or in solitude, and his utterance will be as free as that of the normal speaker. Similarly, *ex hypothesi*, if he is asked to control the movements of respiration, or of the mechanism of articulation in isolation, and for any other purpose than that of communication, he should find it no more difficult to do so than the normal subject. It is the essence of Fletcher's theory that stuttering

arises only in, and is conditioned by, certain relational situations as a pathological social response, so that, in any situation in which stuttering can be eliminated, all the more profound manifestations of the speech-disorder will also disappear. This conception of the disorder becomes a very dubious one if we can produce in the stuttering subject anomalous functioning of the mechanism of speech when speech, either for communication or for any other purpose, is eliminated.

Moreover, if stuttering is physiological in origin, we must expect to find some persistence of the disorder of function even in situations where the sufferer is not required to speak. It is not a final argument against such a hypothesis to point out that manipulation of the social situation in one way or another very often results in facilitation of the stutterer's speech. This holds good also of the organically conditioned aphasias. Hence it has been possible for Travis (5) to argue that such emotional factors are of secondary importance while "in the great majority of cases the basic cause of stuttering is to be sought in some significant variation in the functional integration of the central nervous system." Experiments have been carried out by Blackburn which appear to provide evidence of the persistence of anomalous functioning of the speech-mechanism in stutterers as contrasted with normal subjects, when speech was not demanded, which we should expect to find upon Travis's hypothesis. Blackburn (2) studied the *voluntary* movements of the diaphragm, and of the lips, jaws and tongue in stuttering and non-stuttering subjects. In all four performances he found that the stutterers manifested defective control by comparison with his normal subjects, even although speech was eliminated from the experiments, and the social factor reduced to a minimum. The experiments which are now to be described duplicate the work of Blackburn in this field.

(b) *Experimental procedure.*

The purpose of these experiments was the same as that of Blackburn's, namely, to compare stuttering and normal subjects in respect of control of separate parts of the mechanism of speech during voluntary activity from which speech was expressly excluded. Voluntary movements of the musculature of respiration, and of the lips and lower jaw, were recorded by means of a clockwork kymograph running at a constant speed. The time was simultaneously recorded by a common electrical marker-magnet. Two belt-pneumographs of the type which has been described above were used for recording the breathing movements. In recording the movements of the lips and jaws Blackburn employed two simple

rubber bulbs with walls of different thicknesses, giving a single kymographic tracing for each performance. Apart from the inconvenience and difficulty in manipulating these, his account of the experiments records no precautions of any kind taken to prevent movements of the lower jaw during the recording of the movements of the lips. In the present experiments the apparatus employed for registering the movements of both lips and jaws (Fig. V) was a modification of the lip-recording apparatus described by Gutzmann, in which the recording pencils are directly controlled by the lips.

Since the apparatus acts like a pair of scissors, the movements of the lower lip are registered by the upper stylus, and *vice versa*. For the movements of the lower jaw only one stylus is required, so that when the

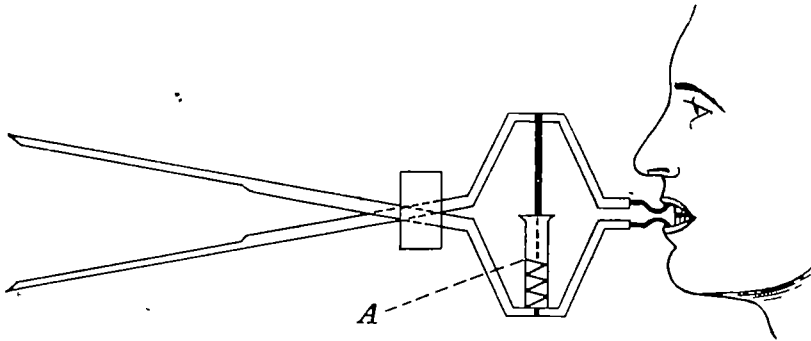


Fig. V.

lower arm of the apparatus has been removed, the upper half of the mouthpiece can be fixed to form a fulcrum or *point d'appui* for the upper teeth. The detachable ebonite mouthpieces are hollowed in the one piece of apparatus as they are in the ordinary lip-key; in the other they are V-shaped so as to be readily gripped between the teeth. Between the arms of the apparatus a small rod and cylinder (*A* in Fig. V), in which a spring is inserted, were introduced instead of Gutzmann's pneumatic bulb. The spring employed in the apparatus for recording the movements of the jaw was considerably stronger and more resistant than that of the lip apparatus.

During the actual experiments the subject was seated before the apparatus with eyes closed in order to prevent his attempting to regulate his performance by watching his own record. The subject was required, during the recording of the lip-movements, to hold firmly between his teeth a small piece of rubber or linen in order to eliminate possible

movements of the lower jaw. The precaution is a necessary one, as the unpractised subject whose natural control of the lips is inefficient tends invariably to resort to biting and chewing. In the breathing experiments movements of both the thorax and the abdomen were recorded by separate pneumographs and tambours, the subject being seated with his back to the recording apparatus. The subjects were instructed to breathe (or to work the lips or jaws) rhythmically or regularly, and fairly rapidly, preserving so far as possible an even speed throughout the performance. In all cases the procedure of the experiment was fully explained and the nature of the performance actually demonstrated by the experimenter, emphasis being laid, as in Blackburn's investigation, upon the requirement of regularity in the speed of movement. Three periods of practice of 15 sec. each were allowed for each subject for each of the three types of performance. Apart from the instruction that movements were to be 'fairly rapid' the subjects were left to set their own speed. In view of the nature of the experiments every precaution was taken to put the stuttering subjects so far as possible at their ease.

On account of individual differences in fatigability, Blackburn stipulated no uniform period of performance or of rest-pauses, instructing his subjects to rest during any experiment whenever they felt that fatigue was interfering with the regularity of performance. In the present experiments, in which most of the subjects were children, it seemed better to set a definite time-limit of performance even if the interval had to be very short. After some preliminary trials with subjects who were not included in the experiments proper, a period of 15 sec., followed by a rest-pause of approximately 45 sec., was set as the uniform limit of the interval of work. The actual records and the reports of the subjects themselves indicate that no significant degree of fatigue was induced in that time by any one of the performances. The voluntary movements of the lower jaw, which were for all subjects the easiest of the three tasks, could indeed have been carried on for much longer without trouble. Ten such records, amounting in all to 150 sec. of actual performance, were taken for every subject on each test. At most six records were taken for any one of the tests on any one day.

The subjects in these experiments were twelve stutterers and twelve normal speakers, three adults and one girl being included in each group. Nine of the subjects in each group were children between the ages of eleven and fifteen years, and so far as might be, an approximate equivalence in chronological age was preserved between the opposite members of the two groups. One of the stuttering subjects failed to complete the

full series of experiments; his records have been disregarded in the discussion which follows.

(c) *Results and conclusions.*

Regarding the handling of the data Blackburn writes: "Since the aim of all trials was to discover the ability of the *O*s in performing voluntary *rhythmical* movements of certain peripheral speech organs, the chief quantitative variable to be recognized was the *regularity* of performance." In order to obtain the required measure of variability he counted the number of movements in every fourth second, finding therefrom the standard deviation in the number of movements performed from second to second. To render the measures comparable for the different types of performance, Pearson's coefficient of variation, $V = \frac{100 \text{ s.d.}}{\text{Average}}$, was calculated for each subject. We have here followed Blackburn exactly with

Table I. *Results from experiments on voluntary control of speech-mechanism.*

Stuttering subjects.

		1	2	3	4	5	6	7	8	9	10	11	12
A. Breathing	M.	5.06	5.26	5.54	7.38	8.10	5.00	6.46	6.44	4.82	—	5.68	7.28
	s.d.	1.06	0.66	1.94	1.67	0.79	1.28	1.50	1.03	0.61	—	0.89	1.70
	V	21.01	12.53	35.00	22.63	9.74	25.70	23.18	15.99	12.76	—	15.63	23.35
B. Jaws	M.	6.39	6.72	5.93	7.56	7.02	5.26	4.24	5.04	5.54	—	5.67	5.40
	s.d.	0.56	0.98	0.86	0.92	1.04	0.95	0.61	0.71	0.36	—	0.67	0.81
	V	8.71	14.61	14.41	12.19	14.82	18.09	14.45	14.03	6.53	—	11.86	15.07
C. Lips	M.	7.93	7.05	6.02	6.52	8.10	6.21	5.73	5.78	5.47	—	5.82	6.16
	s.d.	0.83	0.78	0.98	0.89	1.33	1.06	0.72	0.69	0.58	—	1.41	1.36
	V	10.50	11.08	16.25	13.65	16.39	17.10	12.58	11.91	10.54	—	24.18	22.14

M. Average movements per second.

Table II. *Results from experiments on voluntary control of speech-mechanism.*

Normal subjects.

		1	2	3	4	5	6	7	8	9	10	11	12
A. Breathing	M.	6.82	7.57	5.76	5.49	6.66	7.30	5.41	3.96	5.24	7.20	3.96	6.71
	s.d.	0.51	0.44	0.55	0.56	0.53	0.32	0.54	0.13	0.60	0.48	0.22	0.73
	V	7.48	5.86	9.58	10.18	7.91	4.38	9.94	3.35	11.53	6.70	5.58	10.83
B. Jaws	M.	9.30	8.06	4.17	6.22	4.34	5.45	7.00	2.41	4.13	3.81	4.47	5.19
	s.d.	0.63	0.65	0.17	0.47	1.55	0.76	1.13	0.45	0.63	0.45	0.33	0.28
	V	6.74	8.01	4.02	7.61	14.43	13.97	16.11	18.71	15.22	11.92	7.47	5.33
C. Lips	M.	8.74	7.19	5.07	6.04	4.30	6.96	6.06	1.79	5.68	5.77	6.09	6.47
	s.d.	0.76	0.59	0.89	0.57	0.80	0.78	0.55	0.45	0.74	0.53	0.52	0.57
	V	8.68	8.19	17.61	9.50	18.64	11.22	9.00	24.87	13.05	9.21	8.60	8.88

M. Average movements per second.

the exception that, as the total amount of work done was less than in his experiments, the movements have been counted for every *third* second. Although a double breathing record was taken, the upper chest movements have been disregarded, and only the movements of the diaphragm considered. Similarly, the movements of the lower lip only have been counted, movements of the upper lip being for several of the subjects quite negligible. The statistical results are shown separately for the stutterers in Table I, and for the normal subjects in Table II.

Even superficially considered, the comparative indications of the two tables appear to provide point to point confirmation of Blackburn's findings. More closely scrutinized, however, the results afford an even clearer and more unequivocal demonstration of some of his conclusions than his own data. In particular, they allow us to emphasize and to attempt to estimate the significance of the divergence in the results from the different tests. In summary form, the statistics from our two groups are as follows:

	Stutterers (Mean <i>V</i>)	Normals (Mean <i>V</i>)	Difference
A. Breathing	20.4	7.9	12.5 ± 1.5
B. Jaws	12.9	10.7	2.2 ± 1.1
C. Lips	15.6	12.7	2.9 ± 1.5

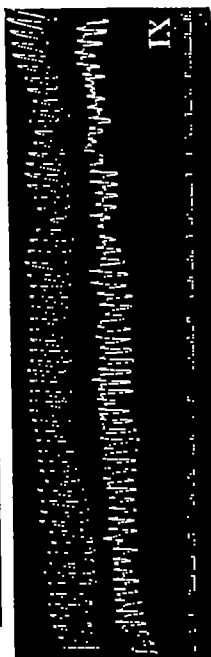
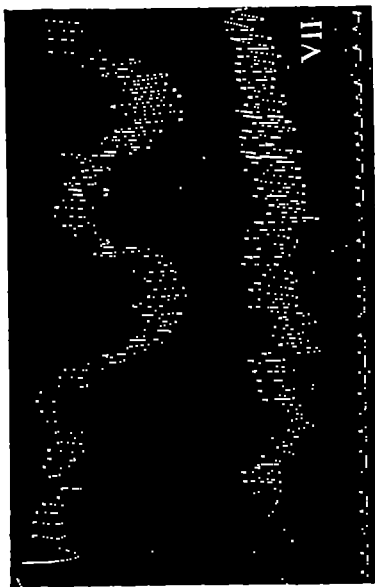
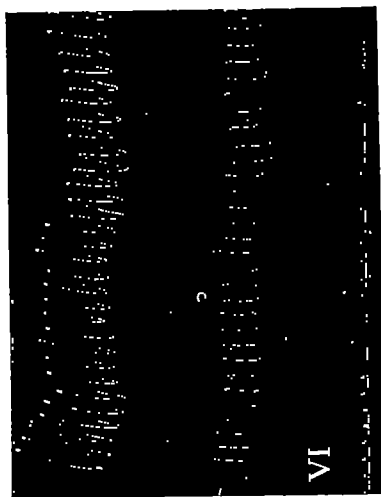
In all three types of performance the stutterers tend to show a higher degree of variability than do the normal speakers. As in Blackburn's experiments also, the difference in favour of the latter is far more marked in the voluntary movements of the diaphragm than in either of the other performances, in both of which the difference between the groups is not considerable. In control of the rhythmical movements of respiration under the conditions of the experiment the difference between the stutterers and the normal subjects is statistically significant to the extent of being eight times as large as its probable error. In the results of the experiments on the movements of the lips and jaws the groups differ only by about twice the probable error.

These results, therefore, offer emphatic corroboration of the conclusion that stutterers show a marked inferiority to normal speakers in *voluntary* control of the mechanism of respiration even when they are not required to utter a word. In control of the rhythmical movements of the lips and jaws they show a similar but much less pronounced deficiency. If, as the results indicate, the differences in the latter types of performance are to be regarded as insignificant, then the experiments serve only to narrow the problem to the explanation of the specific inferiority in voluntary control of respiration. Even if it might be argued in criticism

of the results of the experiments that the ascertained abnormality of function might well be the outcome and expression of a psychological reaction—*anxiety or fear*—to the experimental situation itself, the result still stands as an objection to the theory of Fletcher that stuttering, or the morbid traits of personality of which it is the symptom, is related, not to the social situation as such, but to the social situation in which speech is demanded.

It is in keeping with these quantitative results that our records should for the most part fail to show, except in the experiments on breathing, anything approaching the gross distortions, blockings, haphazard retardations and accelerations, and extreme moment to moment fluctuations in amplitude, which seemed in Blackburn's experiments to be indicative of striking qualitative differences in performance. In so far as he found these in the movements of the lips and jaws, they were probably the results of difficulty in manipulating the rubber bulbs which he employed rather than of real differences in the performances of the subjects. In the tracings obtained in the present experiments, with the exception of those on breathing, the only superficial distinguishing features of most of the records were of the nature of what might be called 'signature differences.' At an early stage in the different tests the subjects tended to standardize not only the speed, but also the characteristic form of the movements, so that the tracings of one subject were as distinct from those of the others as a sample of his handwriting would have been, and that at least in part as a result of the same underlying temperamental factors as are responsible for the differences in script. These idiosyncrasies of performance were most clearly distinguishable in the movements of the lips and were no less characteristic of the normal subjects than they were of the stutterers. The grosser variations in the breathing records of the stuttering subjects are not sufficiently constant for the different individuals to warrant any attempt to classify them into distinct types.

Some typical records are reproduced which illustrate the foregoing discussion. In every case the time is recorded in half-seconds. In the breathing records the lower curve represents the movements of the diaphragm, the higher one the movements of the musculature of the upper chest. In the records of the lip-movements the tracing from the upper lip always appears below that of the lower. Fig. VI is a record of the breathing of the most regular of the normal subjects, characterized by well-nigh perfect rhythm and perfect synchronization of the two tracings. Following it are two similar records by stuttering subjects.



Figs. VI-VIII. Upper curve—Thoracic breathing. Middle curve—Abdominal breathing. Lower curve—Time in $\frac{1}{4}$ sec.
Figs. IX and X. Upper curve—Lower lip-movement. Middle curve—Upper lip-movement. Lower curve—Time in $\frac{1}{4}$ sec.

The first of these (Fig. VII) is distinguished by the large wave which supervenes in the movements of the upper chest, independently of the rhythm of the voluntary panting and bearing no relation to the abdominal movements. The second record by a stutterer (Fig. VIII) exhibits very clearly, like several other tracings by the same subject, an apparently hysterical manifestation which had recently become a prominent feature of this girl's attempts to speak. At a point rather more than half-way through the record there appears a temporary block during which for two full seconds no respiratory movement whatever occurs. According to the report of the subject herself, the stoppage was accompanied by the same distressing sensation of choking which from time to time seemed to check her utterance altogether. Fig. IX shows a characteristic record of the lip-movements of one of the more irregular of the stuttering subjects in this type of performance. At no point in the tracing is there any indication that control has been established or constancy of speed attained. The amplitude of the movements varies quite irregularly, and the opening or closure of the lips is interrupted or momentarily reversed and then continued to the complete destruction of the rhythm. This record may be contrasted with the corresponding one (Fig. X) by a normal subject, which is distinguished by its regularity, both in the speed and extent of the movements, and by the well-defined form of the recurrent movements, especially of the upper lip.

IV. SUMMARY.

In the series of experiments described above the question at issue was that of the relative efficiency of stutterers and normal speakers in voluntary control of the mechanism of speech in situations in which speech itself was not required.

The results of the experiments on respiratory control indicate that the stuttering speakers are markedly inferior to the normals in their ability to maintain voluntarily the rhythm or regularity of the performance. The measures of regularity for the two groups of subjects in this type of performance showed a difference which was more than eight times its probable error. This result affords striking confirmation of the findings of Blackburn from experiments of a similar kind.

In voluntary control of the lips and jaws, although the stutterers are once more inferior to the normal subjects, the differences between the groups are not to be considered significant.

These results when taken together appear to indicate: (1) that although the ordinary rest-breathing of the stutterer may show no

abnormality, there may well be a specific deficiency in his respiratory control; and (2) that respiratory disturbance in the stuttering subject is not confined to or induced by those social situations in which communication by speech is demanded of the sufferer. It was present under the conditions of these experiments in which speech was explicitly eliminated, and the social stimulus reduced to a minimum.

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(Manuscript received 1 November, 1933.)

A NEW PHONIC CHRONOSCOPE.

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- I. *Introduction* (pp. 389-92).
- II. *Description of the 'Heythrop' chronoscope* (pp. 392-6).
- III. *Mechanical construction and details of the instrument* (pp. 396-9).
- IV. *The phonic motor and the electric tuning-fork* (pp. 399-401).
- V. *Certain further uses for the phonic motor* (p. 401).
- References* (p. 402).

I. INTRODUCTION.

IN the very nature of the case, in psychological work such as the determination of reaction times, etc., the magnitude of the value given by any one observation may be considerably influenced by transitory and 'accidental' subjective states having no essential relation to the particular psychological characteristic under observation. To overcome this difficulty, a large number of observations is usually made, and the arithmetical mean of these observations is taken to bear some definite relation to the degree or intensity with which the characteristic in question exists in the person concerned.

In the case of reaction times, discrimination times, etc., this necessity for multiplied observations calls for an easily and quickly operated direct-reading chronoscope. The task of measuring large numbers of graphical or photographic records is a particularly heart-breaking one. Probably all experimenters who have ever worked with a smoked-paper drum chronograph or similar device have registered a resolve never to do so again if it can possibly be avoided.

The Wheatstone-Hipp chronoscope is a direct-reading instrument which has been in use in psychological laboratories for many years now. In many ways it is a highly satisfactory piece of apparatus, giving surprisingly accurate results when operated intelligently and carefully. In the older models, the use of pull-off springs to disengage the clutch when the reaction current was broken constituted a source of error and called for very careful adjustment and control in order to avoid inaccuracy. This difficulty was later overcome by the use of a polarized armature

operated by the current induced momentarily in a secondary coil by making and breaking (or by breaking and making) the current in a primary coil within it. Pull-off springs thus became unnecessary, and both starting and stopping were effected electrically under approximately similar conditions (1).

Even in its improved form, however, the use of the Hipp instrument entails difficulties and inconveniences which are often sufficient to deter experimenters from its employment in cases where the measurement of reaction times is not an essential part of the investigation concerned, but only a useful additional control. Among the difficulties referred to is the necessity for frequent winding-up of the chronoscope. Not only does this involve loss of time and irritating interruptions to the work in hand, but, what is more important, it tends to induce an undesirable element of hurry on the part of the operator, who will naturally try to complete as many reaction determinations as possible for one winding of the instrument. Alternatively, in order to economize winding-up, the going train of the chronoscope may be started and stopped for each determination. This is an added complication, entails undue wear and tear of the instrument, and induces an undesirable tone of tension in the subject under test. The new 'Amerika' chronoscope supplied by Messrs C. H. Stoelting of Chicago is a Hipp chronoscope operated by a spring instead of the usual weights. It is claimed by the manufacturers that this permits the construction of a more compact and easily handled model. The particulars of the instrument which I have been able to obtain do not state whether the spring-drive provides a longer running period than the weight-drive. Further, the Hipp chronoscope, with its high-speed vibrating lamina escapement, produces a peculiar and irritating thin screaming tone. This noise is the more irritating and disturbing in that the necessity for constant rewinding makes it intermittent, and thus constantly forces it on the attention. Moreover, the main dial of the Hipp instrument is very small, and is not easy to read quickly even after considerable practice. A larger dial would, of course, entail the use of a longer indicator needle, and since the moment of inertia increases as the square of the length, the shock of the clutch engagement would disturb the constant speed of the going train unless the power were duly increased by employing larger weights, etc. There is an additional difficulty in the fact that the Hipp instrument does not provide for the resetting of the needle to zero; two separate readings are thus required for each determination.

With a view to overcoming some of these difficulties, and at the same time avoid the expense involved in the purchase of a Hipp chrono-

scope, the writer in 1926 constructed a phonic chronoscope, utilizing for this purpose a simple form of Rayleigh wheel then in his possession. A clutch of the Wheatstone-Hipp type, with stator and rotor crown-wheel, was employed in this model. Further study of the problems involved, and continued experimental work, resulted in the production by Messrs H. Tinsley of South Norwood, according to my design, of the 'Heythrop' Mark III Phonic Chronoscope¹. The tests to which this and a subsequent Mark IV model were subjected, showed that the design was basically sound, but that considerable modifications were called for before the instrument could be considered a really practical proposition. I am indebted for a valuable suggestion to the Rev. M. Trassaert, S.J., Professor of Technology at the University of Tien-tsin, without whose assistance in the laborious work of testing I could scarcely have hoped to succeed. The many improvements which suggested themselves have been embodied in the new Mark V model which it is the purpose of this article to describe.

Before proceeding with this description it will be well to place on record the fact that the 'Heythrop' instrument is neither the only phonic chronoscope nor even the first to be produced. In 1917 Prof. Knight Dunlap published a description of a phonic chronoscope designed by him in the laboratories of the Johns Hopkins University in Baltimore (5, 6). Many models of this instrument have since been produced, and I am informed that they are giving very satisfactory service in various American and European laboratories. Although I first became aware of the existence of this instrument in the course of a discussion at the Copenhagen Congress in August 1932, yet it has several points of resemblance to the 'Heythrop' instrument. However, the special form of clutch mechanism employed in the 'Heythrop' chronoscope, the use of balanced armature coils, the fact that no relays are necessary, and the very wide range of adaptability to various forms of reaction circuits, constitute essential differences which justify the publication of this description.

In 1924 Dr A. B. Wood and Mr J. M. Ford published a description of a phonic chronoscope designed by them for sound-ranging purposes during the War (2, 4). This instrument was friction driven, a small wheel on the indicator spindle bearing against the rim of the heavy fly-wheel of a phonic motor. When the current was interrupted, pull-off springs brought this small wheel to bear against a stationary pad, by which it was brought

¹ The new instrument owes its name to the fact that the bulk of the experimental and testing work was carried out in the laboratories of Heythrop College, Chipping Norton, Oxon.

to rest. One model of the instrument was produced with three dials, so that time intervals could be determined very rapidly, one after the other. In the paper referred to, the authors claim a degree of accuracy considerably in excess of that usually required for psychological work.

For the sake of completeness, and for the information of those who wish for a cheaper form of chronoscope, mention should be made of the possibility of employing a ballistic galvanometer as a time measurer. In such an instrument the deflection is proportional to the time during which a current of constant pressure is applied to its terminals. Such a device has, I understand, been used to standardize the operating-times of the selector relays used in automatic telephone work; its application to psychological purposes is described in an article in the *Journal of Experimental Psychology* of 1917. Prof. R. Pauli, of the University of Munich, has recently designed a very compact form of combined tachistoscope and chronoscope, for use as a universal instrument in the determination of reaction and discrimination times employing either visual or auditory stimuli, and operating with either a motor reaction or a spoken reaction. This ingenious piece of apparatus, whose principal advantages are simplicity, low cost and extreme ease of operation, employs a stop-watch beating forty times a second. For more exact work the apparatus may be fitted with a new and very accurate form of stop-watch which beats 100 times a second.

II. DESCRIPTION OF THE 'HEYTHROP' CHRONOSCOPE.

The 'Heythrop' Mark V Chronoscope consists essentially of a constant speed electro-motor, to which a pointer or indicator needle may be coupled and uncoupled by means of an electro-magnetic clutch of novel design. This clutch arrangement consists of a stator, a rotor and a shuttle-armature, together with their respective windings. Stator and rotor are cylinders of soft Swedish iron, with their inner ends machined to a female taper of 45° . They lie in line with each other, and between them lies the shuttle-armature which is also a short cylinder of Swedish iron, with both its ends machined to a male taper of 45° ; i.e. it is a cylinder with a right-angled cone at each end. The rotor is drilled longitudinally to take a thin spindle of non-magnetic steel, on the inner end of which the shuttle-armature is fixed, and the outer end of which carries the dial indicator needle. This spindle is carried right through the shuttle-armature, and projects a short distance from its rotor end, to fit in a bearing drilled in the centre of the taper recess in the rotor. The spindle serves the double purpose of transmitting the drive from the shuttle-armature to the in-

indicator needle, and of locating the shuttle accurately in line with stator and rotor, in such a way that it is free to slide longitudinally, and be either driven by the rotor, or held fast by the stator.

The stator and rotor lie at the centre of bronze bobbins which form bearings in which they revolve (the stator can be revolved for resetting purposes), and which carry the stator and rotor windings. These windings are permanently in circuit when the chronoscope is in use, and are connected up in opposed senses, so that the inner ends of both stator and rotor exhibit a north polarity. The shuttle-armature is surrounded by (but not touched by) a bakelite bobbin on which three wires have been wound *together*, in such a manner that there are three separate windings of equal length, resistance, number of turns and mean distance from the

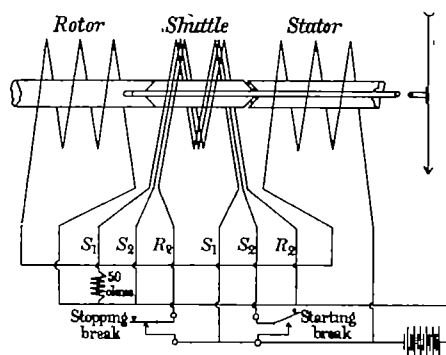


Fig. 1. Wiring diagram.

armature core. The resistance of each of these windings is 50 ohms. One of these windings is usually permanently in circuit, when the instrument is in use; it has a resistance of 50 ohms in series with it, and is connected up in such a way that the stator end of the shuttle-armature has south polarity. Thus when this coil alone is in circuit, the shuttle-armature is held fast against the taper face of the stator. The remaining two shuttle-coils are not permanently in circuit, but may be switched in and out by the stopping and starting keys. They are connected in opposed senses, so that when both are in circuit they neutralize each other and produce no magnetic effect whatever. In Fig. 1 these three coils are marked S_1 , S_2 and R_2 . S_1 produces a field which causes the shuttle to be attracted to the *stator* and repelled from the *rotor*; S_2 produces a similar field but of exactly double the strength of S_1 , since it has no resistance included in its circuit; R_2 produces a field exactly opposed to S_2 , causing the shuttle to be attracted to the *rotor* and repelled from the *stator*.

Operation by two successive breaks. To make the working of the instrument clear, it will be sufficient to describe the sequence of events when the chronoscope is operated on two successive breaks of circuit. We start with all coils in circuit: S_2 and R_2 neutralize each other and so can be neglected for the moment. S_1 causes the shuttle to be held fast against the stator and to be repelled by the rotor. To start the indicator needle, it is sufficient to break the circuit of S_2 . S_1 is still in circuit, and continues to produce a field which tends to cause the shuttle to be attracted to the stator. But R_2 is producing an opposed field of exactly double the strength of S_1 . The resultant field is thus the exact reverse of that produced by S_1 alone. In other words, when coil S_2 is broken, the magnetism of the shuttle-armature is instantly reversed, it is attracted by the rotor and repelled by the stator, it therefore flies over to the rotor and is driven by it. To stop the indicator needle, coil R_2 is broken out of circuit, and coil S_1 is thus left to act by itself, which was exactly the condition of things when the needle was at rest before the instrument was started. The magnetism of the shuttle is thus once more instantly reversed, it flies over to the stator once more and the needle comes instantly to rest.

It is especially to be noticed that the mechanical, electrical and magnetic conditions at stopping are exactly similar to those existing at starting. In this way the inevitable lag at starting is exactly compensated by a lag of similar duration at stopping. In any case, the lag is very short, since the free path of the shuttle-armature is only about $\frac{1}{2}$ – $\frac{1}{4}$ of a millimetre, and the magnetic fields are very strong—the armature therefore moves with a very ‘snappy’ action. This compensating effect, which is a most important feature of the instrument, is obviously quite independent of the current strength passing through the coils; if the current strength were unduly weak, the starting lag would be unusually long, but the stopping lag would be longer by a precisely similar amount. Inaccuracy on this score could only arise in the almost impossible case of a considerable change of current taking place in the interval between the beginning and end of a reaction time.

Operation by two successive ‘makes.’ To time the interval between two ‘makes,’ the sequence of operations described above is exactly reversed. We start with the needle at rest, with only coil S_1 in circuit. Closing the circuit of coil R_2 causes the needle to start; closing the circuit of coil S_2 causes it to stop once more.

Operation by make-and-break. Starting with only coil S_1 in circuit, when the circuit of coil R_2 is closed the chronoscope will start; when this circuit is broken once again, the instrument will stop.

Operation by break-and-make. Starting with all three shuttle-coils in circuit and the needle at rest, the instrument may be started and stopped by breaking and closing respectively the circuit of coil S_2 .

Another method of make-break and break-make operation. The resistance in series with coil S_1 is short-circuited, so that this coil now takes the same amount of current as the other two shuttle-coils. The polarity of coil S_2 is changed by reversing its connections, and it is now plugged in in parallel with coil R_2 , so that both these coils are put in and out of circuit by the same switch. When all three coils are in circuit according to the arrangement described, the needle will rotate; when coil R_2 and the now reversed S_2 are cut out of circuit, the needle will stop, thus operation by either break-make or make-break is easy. An advantage of this method is that by short-circuiting the resistance normally in circuit with coil S_1 , the ampere-turns of the shuttle-coil are doubled and the magnetic attraction is doubled accordingly. This added strength may be used by way of securing greater accuracy, or the normal strength may be used, enabling the current consumed in this circuit arrangement to be cut down by one-half. It should be noted that this arrangement cannot be used to measure the interval between two successive breaks or two successive makes.

Timing the interval between two breaks or two makes of very short duration. In this arrangement, coil S_1 is cut entirely out of circuit, and the instrument operated by means of the remaining two coils, which are in this case connected up in opposed senses, so as to furnish polarity of opposite sign. When both these coils are in circuit, the shuttle is, equivalently, merely a non-magnetized soft-iron armature, and it will be held fast by either the stator or the rotor, according to the position it occupied relative to the stator and rotor when the current was first switched on, and the stator and rotor coils first energized. Let us take the case in which, coils S_2 and R_2 being both in circuit, the shuttle-armature is held at rest against the stator. If the circuit of coil S_2 is now momentarily interrupted, the shuttle-armature will fly over to the rotor and be driven by it. When this momentary interruption of current comes to an end, and coil S_2 comes into circuit again, this has no effect whatever on the position of the shuttle-armature, which continues to be driven by the rotor. A momentary interruption of the circuit of coil R_2 will, however, cause the shuttle to fly back to the stator, and come to rest. Similarly, let it be imagined that the shuttle is held stationary against the stator, and that all the shuttle-coils are out of circuit. In these circumstances, a momentary completion of the circuit of coil R_2 will start the instrument, and a

momentary completion of the circuit of coil S_2 will stop it again. It is scarcely necessary to point out how useful such an arrangement may be for psychological reaction purposes.

Operation by break-make or make-break of very short duration. Let it be imagined that the shuttle is held against the stator, that coils S_2 and R_2 are both in circuit, and that coil S_1 is out of circuit but has its series resistance short-circuited. A momentary break of S_2 will start the chronoscope and a momentary make of S_1 will stop it again. Similarly, if coils S_2 and R_2 are both in circuit, and S_1 , with its series resistance short-circuited and its polarity reversed, is out of circuit, a momentary make of S_1 will start the instrument, and a momentary break of S_2 will stop it. It is important to notice that in all four methods of operation with 'snap' contacts just described, the time interval to be measured must exceed the duration of the first momentary break or make which starts the chronoscope. The stopping make or break may, however, be of any duration. Several other methods of operation are made possible by making or breaking the circuit of the stator and rotor windings.

Special attention is drawn to the fact that whenever the shuttle-armature changes from rotor to stator, it is not only attracted by the stator, but also repelled by the rotor. The same thing takes place *mutatis mutandis* when the shuttle goes from stator to rotor. This feature, together with the copper plating on the shuttle-armature referred to in the next section, entirely obviates any tendency for the shuttle to stick magnetically to the stator, with which it is momentarily in contact before moving over to the rotor; and *vice versa*.

III. MECHANICAL CONSTRUCTION AND DETAILS OF THE INSTRUMENT.

The spindle runs in three bearings, one at each end of the stator, and one at the recessed end of the rotor. The arrangement is shown in Fig. 2. It is essential for precision work that the spindle be dead straight, and that the three bearings lie accurately in line. This ensures that the taper faces of rotor, stator and shuttle-armature come into contact over the whole of their area. Moreover, if one of the three spindle bearings is out of line, even by a very small amount, the spindle will not run freely, and the consequent friction will cause a considerable rise in the mean variation of the readings.

Accurate lining-up of stator and rotor has been secured by simple but robust construction. All the essential component parts of the instrument are solids of rotation, which can easily be produced on a lathe to the required degree of accuracy. The body of the chronoscope consists of a

strong tube cast from bronze, with its internal diameter accurately machined to size. Into the ends of this tube fit the bronze bobbins for the stator and rotor windings, the outer cheek of each bobbin being formed into a flange which is bolted to a similar flange formed at each end of the body-tube. The centres of these bronze bobbins are accurately bored to form bearings for the stator and rotor, which are thus located with their centre axes lying exactly along the centre axis of the body-cylinder. The body-cylinder has a slit extending almost throughout its length, but limited by the flanges at each end. This slit facilitates the insertion and withdrawal of the bobbin wire connections, when assembling or stripping the instrument. The shuttle bobbin, which fits between the

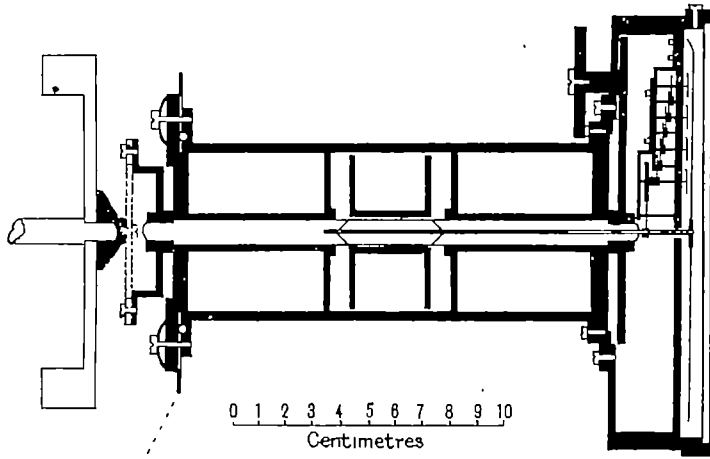


Fig. 2. Section, showing construction.

two bronze bobbins, is made of bakelite, thus avoiding eddy-current effects. Its outside diameter is a close sliding fit in the body-cylinder. Its inside diameter is machined out to such a size that it is clear of the shuttle-armature, which it surrounds, by about $\frac{1}{2}$ mm. This bobbin is held in position by four screws in the body-cylinder. The ends of these screws are cut to a taper, so that the bobbin may be moved longitudinally about 2 mm. each side of its mid-point—a simple but effective arrangement. The taper faces of the shuttle are plated with copper; it is found that this enables the current for efficient action to be cut down considerably.

By means of a knurled hand-knob at the back of the dial the stator can be rotated. This was originally designed to provide a means of re-setting the indicator needle to zero. In practice, however, this knob is used merely as a means of finding the position in which the two spindle-

bearings of the stator are most exactly in line with the spindle-bearing in the rotor. When this position has been found, the stator is not thereafter disturbed, since any alteration of its radial position might involve altering the mean variation of the readings.

The indicator needle is formed from a thin lamina of sheet aluminium; this is rendered rigid by a rib pressed along its entire length. The spindle is carried right through the small boss to which the needle is attached, and projects through a small hole cut in the centre of the glass plate which covers the dial. It is provided with a small knurled knob, by which it is easy to reset the needle to zero with an accuracy of 0.1 sigma. This resetting arrangement is very simple, and it is found to be much more convenient and easily operated than the stator-shifting arrangement originally designed. Of course, while the needle is being reset, the current is cut off from the chronoscope windings; this is most conveniently effected by an ordinary telegraph key, or any similar form of spring-return switch.

Just behind the needle-boss, on the spindle, is a small pinion, which drives a train of pinions operating the five sub-dials which register tenths, units, tens, hundreds and thousands of seconds respectively. The instrument will thus record up to 9999 sec. before the sub-dials begin to repeat their former registration. The main indicator needle can be read directly to 0.001 sec.; and since 0.001 sec. corresponds to a distance of 2 mm. on the circular scale of the main dial, it is easy to read to within 0.0001 sec., by estimation of the last figure. For psychological work, however, such extreme accuracy is seldom, if ever, necessary.

The gap between the shuttle-armature and the stator or rotor respectively is about 0.25 mm. It may be altered by moving the rotor bobbin nearer to, or farther from, the stator. Various more or less complicated contrivances have been tried for this purpose, but, in view of the accurate lining-up required between rotor and stator, none has proved entirely satisfactory. On the instrument in the writer's possession, this adjustment is effected by the insertion or withdrawal of thin copper-foil shims between the flange at the outer end of the rotor bobbin and the flange at the rotor end of the body-cylinder. By this simple arrangement, the rigidity of the whole apparatus is safeguarded and the gap may be adjusted without disturbing the alignment of the rotor. In practice, when the best working gap has been found, further adjustment is not necessary.

In order to avoid undue wear of the rotor-bearing, the drive from the phonic motor is taken to the rotor through a simple form of flexible coupling.

The three shuttle-armature coils and the stator and rotor coils are connected to five pairs of terminals on an ebonite plate fitted to the base of the instrument. This makes it easy to rig up any of the various circuits described in the last section. A sixth pair of terminals is added, to enable the resistance in series with coil S_1 to be short-circuited when required. The writer uses a simple arrangement of plug-in terminals, by which any of the above-mentioned circuit arrangements can be fixed up in a few seconds.

The rotor and stator coils each have a resistance of 12 ohms. Each of the three shuttle-coils has a resistance of 50 ohms. The stator and rotor coils are connected in series with each other, the magnetic field thus provided being amply strong enough for satisfactory operation. Thus, using a pressure of 12 volts, the maximum current taken by the chronoscope with all five coils in circuit, and the resistance of S_1 short-circuited, is 1.22 amperes.

The 50 ohms resistance in series with coil S_1 , and a condenser connected across the motor contacts of the tuning-fork, are fixed out of the way in the base of the chronoscope.

IV. THE PHONIC MOTOR AND THE ELECTRIC TUNING-FORK.

The motor used to drive the chronoscope is one of the well-known 'Ironclad' type, manufactured by Messrs H. Tinsley and Co., of South Norwood, S.E. This is a development of the phonic wheel invented by Lord Rayleigh, and has been designed in such a way as to give considerably more power than is usually developed by such phonic motors (2). It is fitted with a fairly heavy fly-wheel, designed to reduce 'hunting,' a tendency which all phonic motors have to fall into a regular oscillation between a speed a little higher, and a speed a little lower, than the mean radial velocity. The 'angle of hunt,' i.e. the angle of oscillation about the mean radial velocity, cannot exceed $360/N$ degrees, where N is the number of segments in the armature of the phonic motor. Theoretically, this 'hunting' disappears when the motor is supplied with exactly sufficient current to overcome its friction of rotation. Practically, such perfection is never attained, but by means of a small adjustable rheostat in the motor circuit the current may easily be adjusted so that the 'angle of hunt' becomes so small as to be negligible. The heavy fly-wheel makes considerable latitude in the motor current possible without impairing accuracy. The writer has run phonic motors of this description for many hours together on chronoscopical work, and has never experienced the slightest difficulty with them.

The motor can be run, not only on direct current interrupted by an electrically maintained tuning-fork, but also on alternating current of suitable frequency, stepped down to 12 volts by means of a small transformer. The frequency of many sources of public supply is nowadays so constant as to be quite sufficiently accurate for many psychological purposes. Even where it is desirable to employ the more trustworthy tuning-fork, a.c. operation is convenient for demonstration purposes and for preliminary testing work when setting up reaction circuits¹.

The electrically maintained tuning-fork employed is manufactured by Messrs Tinsley. This type of fork was evolved during the War by Dr A. B. Wood and Mr J. M. Ford for use with precision sound-ranging apparatus. It is an extremely satisfactory instrument and embodies several features which are the outcome of lengthy and painstaking research on the part of its designers. By means of heavy adjustable weights of special design at their ends, the two prongs are balanced very exactly with each other. In accordance with the principle of resonance, the fork thus vibrates with a good amplitude on a very small maintaining current (2, 4). Two pairs of contacts are fitted, one on each prong; one for the maintaining circuit and the other for the motor circuit. The contacts themselves, which are made of tungsten, are of robust proportions, and the stationary contact of each pair is fixed in a non-rebound adjustable spring arrangement of very simple but highly satisfactory design. By means of a suitable condenser across the motor contacts of the fork, sparking is eliminated, and fork and motor will run for hours together without the slightest attention being called for. The fork is mounted horizontally on a massive cast-iron base, and the whole is enclosed in a glass-topped wooden case, so that the noise is quite small and quite undisturbing. Actually the fork in action gives a faint low note of rather pleasing tone; it can of course be placed in another room, if desired.

In the instrument in the writer's possession, the motor armature has ten sectors, and the fork has a frequency of 50 double swings per second. The motor, and therefore the chronoscope which is directly coupled to it, revolves accordingly at 300 R.P.M. or one revolution in one-fifth of a

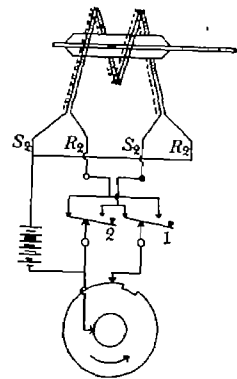


Fig. 3. Switching arrangement for auto-control.

¹ Many electric supply mains are now time-controlled, and can thus be employed to work the synchronous motor. *Note by A. F. Rawdon Smith.*

second. The diameter of the chronoscope main dial is 12.7 cm., and a distance of 2 mm. thus corresponds to a time interval of 1 sigma, giving an open scale easy and pleasant to read.

V. CERTAIN FURTHER USES FOR THE PHONIC MOTOR.

By means of a small pulley fitted to the motor shaft, it is possible to drive kymographs and other similar instruments at a known constant speed. In such cases it is often possible to dispense with the usual time marker and to measure curves against coordinates drawn on the drum paper.

At an early opportunity it is the writer's intention to have a skew reduction-gear fitted to the phonic motor shaft, so as to drive a cross-shaft carrying a disc of about 10 cm. in diameter. The reduction ratio will be 1 in 5, so that the cross-shaft will run at 60 R.P.M. On the face of the disc will be fitted several concentric rows of pegs designed to strike a small electric contact and thus furnish reliable time signals for kymograph markers and for other psychological purposes, such as time-interval discrimination, etc. The advantages of this arrangement over such an instrument as a metronome with mercury contacts, or a clockwork-driven time apparatus, are obvious.

Further, the rim of the disc will have a notch cut in it over a sector of about 18°. A circuit going through a light spring contact bearing on the rim of the disc will thus be interrupted with great accuracy every second. By means of the simple switching arrangement shown in Fig. 3, this contact device can be included first in the starting circuit of the chronoscope and then in the stopping circuit. The interval of 0.95 sec. during which the contact is closed provides ample time for the change-over switches to be operated. By this means the accuracy of the chronoscope may be controlled without having recourse to a fall-hammer, contact pendulum or similar device. If the instrument is registering accurately, the indicator needle should stop exactly where it started. This method of control was used successfully by the writer on a former model of the chronoscope (the 'Heythrop' Mark IV), and a similar method was adopted by Wood and Ford with their friction-drive chronoscope (2). If the instrument passes this test successfully, only one possible source of error remains, namely, that the frequency of the fork is inaccurate, and that the motor is consequently revolving too fast or too slowly. This however may be tested with very great accuracy by running the chronoscope for 5 or 10 minutes against a standard clock or wireless time-signals.

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(*Manuscript received 15 June, 1933.*)

THE BATTLE BETWEEN 'INTUITIONISTS' AND 'PSYCHOMETRISTS.'

By C. SPEARMAN.

At the present day, as is well known, psychology is suffering to an extraordinary degree from dissensions between its several schools. Among these dissensions, especially clear-cut and important is the conflict between two general methods of studying personality. They have been called, misleadingly enough, 'intuitionist' and 'psychometric.' The former relies on more or less direct apprehension of the facts; whereas the latter introduces much statistical elaboration.

The intuitive method has been widely attributed to the psychologists of Germany. Really, it has been dominant at all times and in almost all countries. But in Germany, no doubt, it has gained a privileged position, on account of having allied itself with the doctrine (or doctrines) of Gestalt which there prevails. Fortified by this alliance, the intuitionists in that country, if ever they consider the psychometric method at all, undoubtedly do so with exceptional hostility. The present writer has had special opportunities for observing this adverse attitude, since he has during the last two or three years been taking part in a very extensive attempt at international co-operation.

The method called (mostly by its enemies) 'psychometric' has been principally developed in the English-speaking countries. As already indicated, it does not attempt to abandon any of the direct ways of ascertaining facts, but does claim to supplement these. Moreover, in its more thoughtful versions, at any rate, it adopts a fundamental policy, which it takes to be that of all empirical science. This policy aims at the power of prediction and, for this purpose, at ascertaining *what goes with what*. In consequence of this policy it is that psychometrists have been devising such things as the coefficients of correlation which have been so ill received by the opposing schools.

Now the present article proposes to deal solely—and very briefly—with this unfavourable reception of such statistical devices; it seeks to outline the chief arguments on which the disfavour has been based, and to consider how far these arguments are valid.

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In citing these, use will be made of the recent interesting exposition of them by P. E. Vernon. He writes:

Take for example, the responses to Rorschach's inkblot test, which are determined by the colours of the blots. The author of the test claims that these responses represent impulsiveness and highly toned affectivity in the subject. In order to validate this claim, the psychometrist would obtain the total colour scores of a large group of subjects and correlate them with other criteria (*e.g.* ratings) of impulsiveness, and *would very likely find a negligible or even a negative coefficient. But this result would not disconcert the proponents of the test...* Impulsiveness is not, for the German experimenter, a uni-dimensional and isolated trait, nor a quantitatively definable set of habits of behaviour. It is a quality of reaction which is dependent on and modified by the particular individual's special type of intelligence, by his interests and values, and by his fundamental affective attitude or biotype. The correlation would therefore be accepted as low, not because the colour answers have poor diagnostic validity, but because each subject's affectivity is unique, and can only be understood in the light of the total form-quality of his make-up(1).

In the words italicized (by the present writer) lies the crux of the preceding quotation. The intuitionists themselves in general make no reference to such a possibility of "a negligible or even a negative coefficient [of correlation]." We are left in uncertainty as to whether they have ever thought of such a coefficient occurring, or have repudiated its validity even should it occur. But Vernon, trying to do justice to their case, speaks out for the repudiation; such a coefficient, he says, would in their eyes *not* be disconcerting. Nor does he falter even when such recalcitrant coefficients are the products of his own experimentation and calculation. For he remains, it would appear, still satisfied with this inkblot test when all the correlations which had been asserted of it by Rorschach are found by himself to be really "utterly irregular and statistically insignificant" (2).

On what grounds is it, then, that he so unexpectedly takes the adverse evidence of the correlations to be invalid? It is first and foremost, apparently, because he regards them as being only 'uni-dimensional.' Elsewhere, similarly, statistics are said to have been "developed mainly for the purpose of dealing with uni-dimensional variables." And just here would seem to come into play the doctrine of 'Gestalt,' or Neo-formalism, or perhaps best, 'Wholism.' The correlational coefficients are taken to be incompetent to deal with the structure of the 'whole' or 'total form-quality,' since this involves not one but many dimensions. What the author means by these would appear, from his examples, to consist in such influences as 'type of intelligence,' 'interests,' and 'affective attitude.'

But at this point I would suggest that the intuitionists have by their dislike of correlations been deterred from studying them. In contradiction to the preceding view, I would submit that correlational statistics have been expressly devised in order to deal with *multi-dimensional* variables and, furthermore, in order to distinguish these from and integrate them with the uni-dimensional ones. This seems to me to be not only evident but fundamental already in the original work of Bravais. As much may be said of the great contribution of Udney Yule, which has dominated all theory and practice of correlations subsequently. For this work of his has brought correlations into a system of 'orders' which range from those which are 'total' (i.e. deal with wholes) down to those which are more and more 'partial.' Or take the fine correlational studies initiated by Garnett. This work not only is still based on representing indefinitely numerous dimensions, but now goes so far as to represent these dimensions geometrically.

Here in this matter of personality, then, we would appear to have a drastic example of the fundamental weakness of the Gestalt doctrinaires. *They use the terms 'Gestalt,' 'Ganz,' 'Total' and 'Whole' as a sort of taboo on further enquiry and analysis.* Whereas, as it seems to the present writer, *wholes can no more be studied efficiently without references to parts, than parts without wholes.* The task of science, I suggest, is not to prefer either, but to integrate the two. In comparison with this fundamental misunderstanding of psychometry by the intuitionists and most Gestaltists, little importance need be attached to the many minor ways in which they show themselves to have studied statistics inadequately. We will briefly cite a few examples. Thus, the traits with which correlations deal are taken to be necessarily of a quantitative nature, linearly correlated, and even normally distributed. Not one of these alleged limitations has any basis in fact. Again, we encounter ill-founded criticisms of the statistical use of large numbers. Here is one instance—"mere frequency of occurrence has never impressed him [the supposed German] as a criterion of truth (1)." This is indeed a surprising way of regarding the evidence that frequency of occurrence can and does afford. What a glimpse of estrangement from all such studies as those of induction, sampling and so forth! Still more naïve would appear to be such statements as that traits may be "so subjective and equivocal that psychometric methods almost inevitably blur, and therefore fail completely to confirm, whatever truth there may be in the German's intuitive generalizations." In point of fact, as soon as any individual trait whatever can be stated with any degree whatever of accuracy, or of probability, so soon does this trait become

amenable to statistical handling. This handling does not produce blur. Nor can it even be said to fail in treating this. On the contrary, it is only too successful. It shows up the blur!

Among recent publications bearing conspicuously on our present topic is an elaborate and interesting attempt of Lewin to represent the calculation of frequencies and averages as a method opposed to that of establishing scientific laws(3). Of several fundamental points on which this article seems open to objection, salient here are the inadequate statistics which alone are depicted by him and, apparently, alone are taken by him to be possible. In particular, he seems not to have noticed that the frequencies and averages, far from opposing the laws, should be among the most potent tools towards establishing them.

So far we have been obliged to defend the psychometric method against some limitations which seem to have been charged against it ignorantly. But we hasten to concede that, on the other hand, it does commonly suffer from other limitations only too genuine. In the first place, there is a mischievous tendency to suppose that exactitude of statistical handling can compensate for inexactitude or ineptitude of observation or of experiment; whereas neither statistics nor anything else can make a silk purse out of a sow's ear. In particular, the very fact that statistics deal with multi-dimensional variables tempts investigators to dispense with the good experimentation that varies each influence separately.

In the second place, statistical statements, since they are primarily concerned only with coexistence, have little value for us until we know (well enough for recognition at any rate) *what* thus coexists. This limitation, of course, affects even physical science, but not so grossly. In psychological statistics, the disproportion between the wealth of figures and the poverty of their meaning is often pitiable.

In the third place, although the coexistence of the terms seems to be their sole interrelation that is absolutely indispensable for predictive purposes, still science if not killed is at least paralysed by the absence of knowledge (or hypothesis) of other relations; such as those of structure and of causation. And here statistics may indeed lead us into serious trouble. Suppose that X is correlated with Y . Does this prove that X causes Y , or that Y causes X , or that both are effects of one and the same cause, or what? In consequence of such doubts, statistical data may be woefully equivocal; they may admit of widely different interpretations.

This brings us to the wise words of Udny Yule, who says that the main function of statistics is to verify conclusions reached already other-

wise. But most fallacious is the twist sometimes given to these words by taking them to mean that the statistics are superfluous. So far as this goes, psychology is only in the same boat as the other empirical sciences. Save for some mere happy accidents, *all* discoveries have been surmised beforehand. But out of countless surmises only a few have the good fortune to be verified. Many come, but few are chosen. This is the great ordeal of science. The trouble with the intuitionists is that by them this ordeal is shirked.

But here again there is grave fault to be found also with many psychometrists. Since, as we have seen, discoveries originate almost solely from verifying surmises, then what shall we say for the prodigious array of figures wherein foregoing surmises are conspicuous by their absence? Such researches are undertaken without any theory to verify; without any meaningful question to put to Nature; in short, without any ideas behind them, but only a hope of picking up these by chance.

Thus the account is in a way balanced. The intuitionist tries to make ideas work without mathematics. The psychometrist, mathematics without ideas. When will both learn that two legs are better than either alone?

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(*Manuscript received 15 December, 1933.*)

A GENETIC STUDY OF MOTIVATION UNDER CHANGING AUDITORY SITUATIONS¹.

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I. STATEMENT OF PROBLEM.

ONE general conclusion can be drawn from all experimental work upon motivation where auditory stimulation is used in the incentive situation: motivation effects are highly individual. It is therefore necessary to work with observers individually and over a fairly long period of time in order to understand the conditions of motivation and to secure a genetic picture of motivation in both its behaviour and conscious aspects. This was the plan of the experiment reported here, which was arranged to answer, if possible, the following questions:

What is the effect of changing an auditory pattern of pitches upon accomplishment in a psycho-motor repetitive task? What is the effect of the removal of a familiar pattern? What is the effect of changes in the timing of the auditory pattern? What is the relation of any changes in accomplishment which are found under these conditions to those resulting from a change in verbal instructions? What happens in awareness at the

¹ This investigation was performed in the Psychological Laboratory of the University of Cambridge during 1931-2. Acknowledgment is made of the assistance of K. Edwards, M. Dutton, R. Harbinson, and E. L. Trist, who were the observers in the investigation.

DIAGRAM OF ARRANGEMENT OF APPARATUS

NOTE:
 1 BELLS OF MEG & DOMINANT
 2 BELL AS INDICATED
 3 LOW SHEEPBENT MEG 194 DV
 4 HIGH SHEEPBENT MEG 191 DV
 5 ROUND GONGIGE MEG 193 DV
 6 LOW CHURCHIGENT MEG 174 DV
 7 HIGH " " G.E. MEG 104 DV

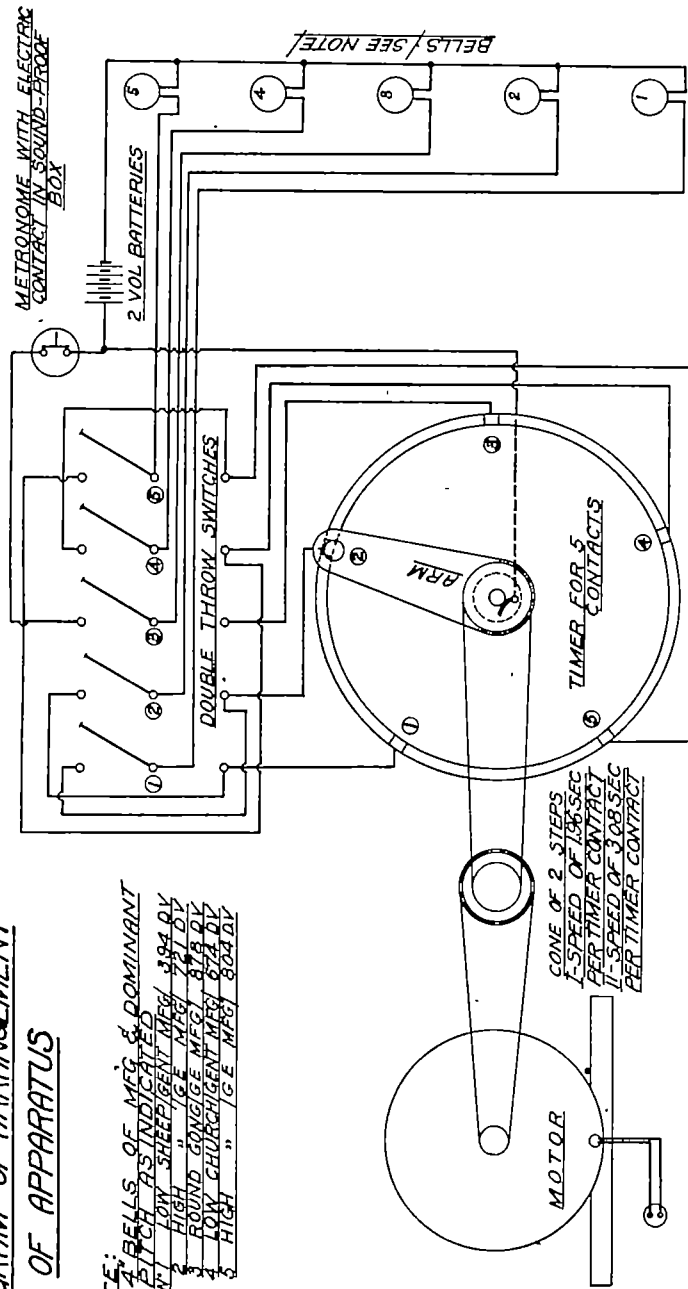


Fig. 1. Diagram of arrangement of apparatus.

time of these objective changes and what is the relation of any changes in awareness to accomplishment? How is the intent of the observers influenced by the objective changes?

II. THE EXPERIMENTAL CONDITIONS.

The development of motivation in four observers was studied over a period of ten days (for about two hours a day); the first two days consisted of work with the original auditory pattern and the following eight days with four changes in auditory stimulation. The original auditory pattern contained five bell tones (see Fig. 1 for arrangement of apparatus) of different pitches¹, played in regular timing at the rate of 1.96 sec. per gong. The first change in the auditory stimulation was one of pitch relations, and consisted of reversing gongs 1 and 2 and gongs 4 and 5. The second change consisted of increasing the intervals between bells from 1.96 sec. to 3.08 sec. per gong. The third change was continuous and was effected by playing gong 3 (round gong) at the rate of 36 times per minute by means of the metronome attachment (see Fig. 1), causing this bell to change relations with the other four which were being played as in the original pattern. The fourth change was a complete elimination of the auditory stimulation.

The repetitive task for a daily observation consisted of four addition tests, each of 1200 problems, with totals of not more than nine. The tests were prepared to meet the conditions of the experiment. A test was timed in two parts of four minutes each, with a rest period of one minute between parts, and a written report of conscious motivation followed the test². The experiment was planned to relate any possible changes in accomplishment to the changes in stimulation and to have a close check upon the development of the intent at the same time. The observer's report usually consumed about two minutes and was directed by a set of written instructions asking for information of any conscious urges, hindrances, and dominant awarenesses during the task.

Work with the original auditory pattern was continued throughout the first two daily observations, until the observers were familiar with it. Experimental changes took place on the following days at the third test.

¹ The vibration frequencies reported in Fig. 1 are for the dominant tones of the bells, but numerous other tones, beats, and noises are present in commercial bells such as these.

² *Speed Addition Test*, published by the New York University Press Book Store. Copies in Department of Psychology, University of Cambridge, and New York University University Heights.

On the third day the pattern of pitches was changed and continued for the third and fourth tests. On the fourth day the observation began with this accompaniment and the change was back to the original pattern at the third test. On the fifth day the change in regular timing took place at the third test. On the sixth, the observation began with this accompaniment and the change was back to the original accompaniment at the third test. On the seventh and eighth days the continuous change was likewise dealt with; and on the ninth and tenth days the elimination situation.

The instructions, which were read only at the first observation, directed the observer to 'work as rapidly as possible.' The time of each test was constant and known to the observer so that he might note his accomplishment in each test and self-rivalry be developed. But no knowledge of records of other workers was given.

The development of intent is traced by means of the observers' reports and related to the measures of accomplishment for each individual observer. The statement, made above, that motivation effects are highly individual, is borne out by the present results. Each of the four observers is different in his manner of following the instructions to 'work as rapidly as possible' in the situation presented to him. A measure of this motivation is found in the changes in accomplishment of these observers. The descriptions of the intent indicate the point at which the auditory stimulation is affecting the individual. The results from these four observers would seem to illustrate what goes on in similar incentive situations with larger numbers of observers, where results are accepted as if all observers were working with the same motivation.

The observers worked on a table about 5 ft. away from the auditory stimulation, which was behind a screen, and with their backs to it, in a room 12 ft. by 16 ft. The ringing of the large 4-inch gongs was increased in intensity by increasing the current (see Fig. 1) necessary to sound the bells¹. Hence the stimulation was very insistent and there was a great deal of noise in the tonal complex. It is possible that the pitch change in stimulation of the third and fourth observations did not stand out in awareness because of this fact. It is also possible that there were objective changes in the situation due to weakening of the hammer springs of the gongs, which had to be changed on occasions because of the extra demand upon them brought about by the increased current.

¹ Six volts required, ten volts used.

III. THE DEVELOPMENT OF THE CONSCIOUS INTENT.

It is found that the observers worked with different intents throughout the experiment, where forty reports were secured. Observer A has the general intent to 'carry-on' and maintain previous established levels of performance; B soon develops the intent to work at a comfortable pace; C develops the intent to reach the bottom of the page (in four minutes) by gradual daily increases; D also develops an intent to reach the bottom of the page, but by means of all accessible incentives, of which the auditory changes are reported as the important ones. These four intents were developed out of the initial instructions to 'work as rapidly as possible.' There follows an abstracted statement of each observer's report as it concerns the development of a general intent to do the task under the conditions of the experiment. Special intents of short duration accompanying changes in the auditory situation will be discussed later.

Observer A. General intent. In the very first test there is an urge "to overcome noise stimuli in the experiment." In the second it is "to get to the bottom of the page before the signal to stop is given." At the third the observer is not aware of any specific drive except "to get the task finished and to overcome obstacles. The beginning of the task is the source of the motive to complete it." The "noise hinders work." Day-dreaming is regarded as a hindrance. He says in the last test of this first observation: "The bells were a hindrance which I tried to overcome." From this observer's report in the first observation it is evident that any increase in working effort due to the auditory stimulation is in shutting the latter out of awareness.

This urge to shut out the noise accompaniment becomes more definite in the first test of the second observation, when it is said that the intent is "to overcome distraction and beat my previous performance." The noise of the bells is spoken of as a hindering factor. Again, in the second test the urge is "to keep up performance to a high level." The intent is stated in the third test as "trying to accomplish the task as rapidly as possible, by excluding all hindering factors, such as the quality of the tone and intensity of individual bells, variations in timing, growing amount of fatigue and day-dreaming."

In the early tests of the third observation this observer says that: "In general I am performing the task as quickly and accurately as possible, and to do so I am endeavouring to concentrate solely on the task in hand and to exclude everything else." In this there is "a certain amount of conscious effort to overcome distractions," of which the outstanding one is given as the auditory stimulation. The pattern of arrangement "seems to be a factor tending to obstruct the performance of the task." Other distractions are given here as the "repetitive character of the work," "flight of fancy," and "fatigue of arm." At the fourth test is found the statement that there is "no marked specific urge except to get it over," and that "day-dreaming was a hindrance as I was aware of slackening speed."

In the first test of the fourth observation he says: "I am putting forth a tremendous effort to exclude sound of bells and to concentrate solely on addition" and I wish "to do as well as other observers"; also, I "desire to surpass my previous perform-

ance." This is a statement of this observer's general intent at its highest point. In the first test of the fifth observation it is stated that there is "no significant urge, am just doing it because I have to do it. The task itself provides an urge which is fairly distinguishable, and I manage to exclude the distractions fairly well." He speaks again in the fifth observation of his intent "to maintain previous performance and exclude external stimuli."

The intent grows more vague in the sixth observation and is stated as a "desire to complete the task. I have not made any definite effort to eliminate day-dreaming since I have found that I can carry both without interfering with the other." In Observation VIII an urge is reported "to put up the best achievement yet," but this is not very lasting as the observer says in the same test that his "desire later on was to get along all right," by which he seems to mean to keep up his performance to past achievement. In Observation IX there comes an "awareness of a fairly good performance in the first part of the test and so some effort to maintain and surpass this," but this intent is transitory and the observer slips back to his old intent, as stated in the last observation. "There is no urge except to accomplish the task."

Observer B. General intent. Observer B immediately re-formed the instructions into an intent of "competition with myself to see how many rows I can complete," and "attempt to keep my mind off the bells to keep from getting annoyed." This observer speaks of being "a wreck" after the first observation and coming to "the decision not to fight against the bells so hard or to hurry." The work becomes more passive in the second observation and the intent is "acceptance of bells to-day. Adding right along with a great deal of irregular thinking. I am not hurrying because I do as much when I don't." The bells are very disturbing at the beginning of the third observation. They "seem louder, and are certainly more painful this morning." They act "as a constant interruption." I have "no success in excluding them." An "urge to think of other things in order to escape the noise and unpleasant task" developed in the fourth observation. In this observation it is reported that "I am trying (though not very hard) to do as much as I can to-day, am feeling better." There is a "wonder if I could do the whole page." In the fifth observation comes the renewal of the urge "not to try too hard since the effort is a strain at the time and makes me feel unpleasant afterwards." A certain working speed which does not cause feelings of unpleasantness has been established by the sixth observation. "The motive is to exclude bells, relax and think of other things." But as she reports in the seventh observation: "Sometimes though, I find myself hurrying to get the test over, which, of course, is no use."

Observer C. General intent. Observer C accepted the auditory stimulation as an indifferent extraneous situation in the first observation and stated: "I feel that the noise would be regarded as unpleasant under ordinary conditions. Here I do not mind it." In the first test of Observation II the report is as follows: "I am conscious of pushing on from left to right along the rows," and in the third test, "I am aware of the task to be performed. Tones are part of the general situation in the background all the time, and I am concentrating on hand movement, co-ordination of hand and eye, and occasionally thinking of other things." Into test three comes the report that "towards the end a violent effort was made to get another row of figures done before the stop-signal," and again in the fourth test, "I attempted to get farther down the page than usual, a feeling of urge to get the figures down." This observer "forgets

about the bells during work" and reports in the second test of the third observation that it is "more of a strain than last time because I am trying to work harder." There is "a feeling that it would be grand to get to the bottom of the page before the signal." In the third test of this observation is a report of development of methods of work and the statement of a feeling that "I cannot improve in speed because it is a case of writing more rapidly." In the fourth test there is the report that "now I look on the whole task as an effort to obtain speed." A statement of motive is made in the fifth observation as follows: "I aim to perform series of additions rapidly, paying little attention to outside conditions as far as possible, and try to get down to the foot of the page before the stop-signal." She reports being "all set to go at the beginning," and at the end of the test "trying to get through one more line before the signal comes." Her intent has become "to get farther down the page each time, I regard each trial as one in a series which is training so that finally I shall be able to complete the page in a given time without undue effort." Reporting progress, she says that "the task seems easier than formerly, less of a strain. The combination of sound, light and my own activity is one complete whole. I find that I am quite anxious to get going at each new test to see what I can do. Toward the end of each part I feel that time must be nearly up. I am worried in case I haven't got as far as usual and there is a feeling of relief when I recognize that I am somewhere near the point where I stopped last time." In the seventh observation it is reported that the "task seems to have more conflict involved than earlier. Probably different methods conflicting." In Observation VIII she says "I am working more slowly to get steadiness, using it as practice," and she is aiming to complete a certain row of figures each time. In the ninth observation the aim is to secure "complete smoothness at uniform rate," which is a description of her general intent.

Observer D. General intent. This observer reports in the first test "a considerable urge to get as much done as possible and to do more in the second half than in the first." During Observation I he is highly conscious of the auditory stimulation and analyses it into its parts, and he considers that the accompaniment is a hindrance to the task. There is continued analysis in the second and third observations and little development of intent to accomplish the task as rapidly as possible. Attention is upon analysis and later upon shutting out the auditory stimulation from awareness. In the fourth observation awareness turns to the adding process. The aim is stated "to hasten speed by approximating a continuous flow of writing. The great urge is to get to the bottom of the page if possible, it being felt that some new technique could produce a considerably greater amount of work." There is the "urge to set a new standard for myself." This observer experiments with methods and builds up a fairly complex intent to reach the bottom of the page in the time allowed. He reports "an urge to add with might and main so as not to be thought a half-wit, and a general desire to be useful in the experiment. The adding is now begun with the hope of reaching the bottom of the page." This observer says: "I am endeavouring to add the numbers at a maximum speed, to devise the best technique for this and to add the whole page in the given time. Having got to within three or four lines of the goal the urge is greater and a feeling of 'now or never' is present." This observer frequently reports that he is waiting for the appropriate mood to accomplish this aim. In Observation V comes the statement of an "urge to do a whole test at a spurt-pace, as one gradually learns to

run the quarter-mile at the spurt-pace of the hundred yards." In the next test he tries this, but fails and returns to another method of "faking up stimuli from imagination to keep himself going." In the seventh observation "there was a feeling that the limit had been reached," but in the eighth observation "the desire to get to the bottom of the page has become supported by the belief that it could be done. I feel myself waiting for the ideal conditions to turn up when I expect the extra little bit of inspiration needed to do it."

These abstracted reports of general intent give the picture of conscious motivation for these observers. We know nothing about the levels of motivation which may have been established in past performance, but it would seem reasonable to assume that the motivation in this experiment would be developed upon the motivation expressed in similar tasks in the past. Table I gives the objective evidence of accomplishment during the experiment. It shows the differences between the highest and lowest (first test) scores of each of the four observers. The 'corrected per cent. increase' is based upon the lowest beginning score (of Observer D) and is used to equalize the unknown factor of a previously established motivation level in this kind of work. The assumption is made here that these observers, who were of similar general mental ability, have the same ability to do simple addition problems and that the differences at the beginning of the experiment are differences in starting motivation. This may or may not be a correct assumption, but it serves to illustrate what are possible comparative motivation effects in different individuals. The differences in accomplishment in this experiment are treated as motivation effects rather than as ability or practice effects. The simple addition tests, used as the task in the measurement of accomplishment, were prepared for the experiment in an effort to hold these ability differences constant. It appears from evidence accumulated in various experiments that practice, by which is meant the effect of the repetition of a task, plays but a small part in the acquisition of speed. Differences in accomplishment, such as are observed here, appear to be caused by motivation. This would lead one to accept the 'corrected per cent. increase'

Table I. *Differences between individual observers in accomplishment throughout forty tests of 1200 addition problems.*

	Observers			
	A	B	C	D
Lowest score (first test)	706	778	690	606
Highest score	963	940	1129	1154
Difference	257	162	439	548
Per cent. increase	36	21	64	90
Per cent. above lowest (Observer D) at start	17	28	14	0
Corrected per cent. increase	53	49	78	90

for comparative purposes, and assume that differences in motivation exerted in the first test were responsible for the beginning differences.

A more complete genetic picture of motivation throughout the experiment is seen in Table II, which shows the increases for each observer by observation. These increases are based upon the average scores for each of the ten observations, which were composed of four tests each. The figures in parentheses are for extra observations. The general correspondence of intent and accomplishment is noted here in detail. After the first few observations Observers A and B show no marked increases. Observer C shows highly consistent and regular increases throughout the experiment, which is in exact agreement with her intent. Observer D is more irregular, but the increases continue throughout this experiment, where the intent is to use every possible incentive to increase accomplishment.

Table II. *Showing increases in average scores for observations throughout experiment (increases based upon previous highest average).*

Observations	Observers							
	A		B		C		D	
	No.	%	No.	%	No.	%	No.	%
II	47	19.7	10	4.7	17	3.9	68	15.8
III	16	6.7	5	2.3	23	5.3	32	7.4
IV	23	9.7	65	30.4	67	15.5	110	25.5
							(46)	(10.7)
V	24	10.1	0	0.0	84	19.4	28	6.4
VI	8	3.4	0	0.0	32	7.4	0	0.0
VII	0	0.0	0	0.0	17	3.9	39	9.0
VIII	0	0.0	0	0.0	53	12.3	15	3.5
IX	28	11.8	0	0.0	58	13.4	42	9.7
X	0	0.0	0	0.0	0	0.0	51	11.8
					(38)	(8.8)		
'New intent' test	92	38.6	134	62.6	43	10.0	—	—
Total	238	100.0	214	100.0	432	99.9	431	99.8

IV. EFFECT OF A NEW INTENT.

New instructions were introduced at the conclusion of the tenth observation to three of the observers (A, B, C) calling upon them to put forth all possible effort to complete the test in the time allowed. Observer D had already completed the last half of the last test in the time allowed, which is 600 problems in four minutes. For this reason the 'new intent' test was not administered to this observer. The three observers were told that they had already made a very high score, that they now worked very rapidly, but that this was the last test, and that they were to put forth every effort to get to the bottom of the page in the time

allowed. No one knew the record of the others. These instructions came at the conclusion of the tenth observation, when each observer had had over 300 minutes of adding practice. The auditory situation was eliminated as in the last observation. All observers felt that they had already accomplished as much as they could, but accepted the instructions to make another try. Observer B, in particular, argued that she was sure she had done all she could.

The increases in scores for the 'new intent' test are shown at the bottom of Table II. Observer C makes about her regular increase. There is no added conscious motivation indicated in her report over her previous tests in which she had been working with what evidently was her maximum drive. Observer A makes almost 40 per cent. of the increases which he made throughout the entire experiment. That is to say, in the last eight minutes of work he makes four-tenths of the increases which he made in 320 minutes' work. This increase is 7.89 times the s.d. of this observer's normal fluctuations throughout the experiment. He reports his intent during this test, as follows:

"In this test I tried to break all previous records and get to the bottom of the page if possible. I had a feeling, however, that I was not actually going to get to the bottom of the page, but there was a tremendous urge to surpass previous accomplishment."

Observer B makes more than 60 per cent. of her increases in this test. Yet at the beginning of the test it was necessary to spend some time in convincing her that she could do better. This increase is 7.13 times the s.d. of this observer's normal fluctuations throughout the experiment.

These tremendous increases in accomplishment, upon the acceptance of a new intent to greater effort by Observers A and B, imply that accomplishment is conditioned by the conscious intent and that motivation rather than practice was responsible for the advance. The results from Observer C corroborate this conclusion. The general intent of Observer C was to increase accomplishment with every test. There are regular increases, and upon the request for greater effort the report of intent indicates that the situation is much the same. The increase in accomplishment is uniform. Observer C's increase with the new intent was just 0.39 times the s.d. of her normal fluctuations.

The conclusion already noted in the research of other investigators is illustrated in the cases studied here. There are wide individual differences in intent to do a task under the same experimental conditions. Also, correspondence is present between the accomplishment and the individual manner in which the intent to do a task is developed.

Table III. *Accomplishment results with changes in auditory situation.*

Individual observers	From original to new situation			
	Pitch Observa- tion III	Timing Observa- tion V	Continuous Observa- tion VII	Elimination Observa- tion IX
For Observer A				
Intent development	0	2	1	- 1
Score change	- 4	32	- 26	- 27
Corrected score change	14	50	- 8	- 9
Ratio of change to s.d. of norm	0.78	2.78	- 0.44	- 0.50
For Observer B				
Intent development	0	1	- 1	1
Score change	62	19	- 53	13
Corrected score change	47	4	- 68	- 2
Ratio of change to s.d. of norm	1.57	0.10	- 2.27	- 0.07
For Observer C				
Intent development	0	2	1	1
Score change	21	20	43	26
Corrected score change	4	3	26	9
Ratio of change to s.d. of norm	0.13	0.10	0.84	0.29
For Observer D				
Intent development	- 2	1	3	3
Score change	- 51	51	74	244
Corrected score change	- 33	69	92	265
Ratio of change to s.d. of norm	- 1.36	2.68	3.71	11.04
Individual observers	From new situation to original			
	Pitch Observa- tion IV	Timing Observa- tion VI	Continuous Observa- tion VIII	Elimination Observa- tion X
For Observer A				
Intent development	0	0	2	0
Score change	5	- 15	18	- 7
Corrected score change	23	3	36	11
Ratio of change to s.d. of norm	1.28	0.17	2.00	0.61
For Observer B				
Intent development	0	0	- 1	- 2
Score change	21	20	- 19	- 64
Corrected score change	6	5	- 34	- 79
Ratio of change to s.d. of norm	0.20	0.17	- 1.13	- 2.63
For Observer C				
Intent development	0	0	0	0
Score change	42	27	- 16	21
Corrected score change	25	10	- 33	4
Ratio of change to s.d. of norm	0.83	0.32	- 1.06	0.13
For Observer D				
Intent development	2 (2)	2	1	1
Score change	110 (67)	68	133	- 27
Corrected score change	128 (85)	86	151	- 9
Ratio of change to s.d. of norm	5.32 (3.42)	3.46	6.29	- 0.38

V. THE EFFECT OF THE AUDITORY SITUATION.

The relative effects upon accomplishment of the various auditory situations are shown in Table III. Accomplishment with the original

auditory pattern is the standard for comparison. Changes in accomplishment are related to the reports of the individual observers. The first row of figures, for both the changes to the new auditory situation and back to the original, is composed of estimates of the development of the conscious intent when this change took place. Zero indicates no change in the development of the intent at the time of the change in auditory stimulation and numbers 1, 2 and 3, in plus and minus quantities, are used for degrees of this development. These ratings are given as long as there was development of the intent and whether or not the observer related his intent to the changes in the auditory stimulation.

The second row of figures (score change) shows the changes in accomplishment, in plus and minus quantities, between the third test of the observation, where the change in auditory stimulation took place, and the average of the first two tests. The average of the first two tests is used as the measure of normal accomplishment for the day's observation. This 'score change' is then related to the average normal change between tests where the original auditory situation is present, *i.e.* the change from the first to the second test in Observations III, V, VII, IX, where the observations begin with the original auditory situation, and between the third and fourth tests in Observations IV, VI, VIII, X, where the observations conclude with the original auditory situation. The difference between the score change, where there is a change in the auditory situation, and the average of the score changes between tests where the original auditory situation is present, is shown as the 'corrected score change.' This is the amount of change in score which may have been caused by the change in the auditory stimulating situation. The 'corrected score change' is related to the s.d. of the changes in score between the control tests, and is given as a 'ratio,' which if approximately three indicates that the change in accomplishment is statistically reliable and greater than any of the normal fluctuations in score.

The normal change in score was computed as the average of the difference in scores between the two tests in each observation where the original auditory situation was present, *i.e.* the first two tests for Observations III, V, VII and IX and the last two tests for Observations IV, VI, VIII and X. For the four observers this figure is as follows: Observer A, mean -18 , s.d. 18 ; Observer B, mean $+15$, s.d. 30 ; Observer C, mean $+17$, s.d. 31 ; Observer D, mean -18 , s.d. 19 . Differences between tests with the original auditory situation coming at the beginning of an observation and at the end of an observation tend to be in an opposite direction. Thus these s.d.'s are averages of two s.d.'s, one for each position. For Observer A the beginning mean is -25 , s.d. 25.16 ; the concluding mean is -11 , s.d. 10.58 . For Observer B the beginning

mean is -6 , s.d. 36.17; the concluding mean is $+35$, s.d. 23.59. For Observer C the beginning mean is $+13$, s.d. 37.90; the concluding mean is $+21$, s.d. 24.75. For Observer D the beginning mean is -5 , s.d. 19.40; the concluding mean is -30 , s.d. 28.33.

Table III shows the accomplishment effects accompanying the different kinds of changes in auditory stimulation, that is, upon a change in pitch pattern (Observation III), a change in the timing or intervals between the individual tones (Observation V), a continuous change both in pitch relations and timing relations (Observation VII), and finally the elimination of auditory stimulation (Observation IX). Underlying all the possible individual effects of the changes in auditory stimulation is the general effect of the auditory stimulation itself. It is seen that none of these auditory situations was accompanied by changes in accomplishment by Observers A, B and C which are statistically reliable. Observer D is an exception, and six out of the nine changes in the auditory situation were accompanied by significant changes in accomplishment.

An analysis of the reports of these observers should indicate any parallel between changes of intent and changes in accomplishment. It has been shown that differences in accomplishment between the individuals in this experiment were conditioned by the general development of intent to do the work. Here we have the evidence of the correspondence of changes in intent and in accomplishment at the time of changes in the auditory situation. This correspondence is shown in correlations by the Rank Difference Method for eight auditory changes. These values are given in Table IV. As isolated measures these correlations are of little

Table IV. *Correlations between changes in score and estimated changes in intent.*

Observers				Average
A	B	C	D	
0.70	0.51	0.17	0.69	0.52

value. But viewed in connection with the descriptions of general intent they are significant and they are used as rough measures of relationship. Observer A had developed an intent to maintain a previously established level of performance. At the change in timing of the auditory stimulation (Observation V) he reports that:

"There were decidedly longer intervals in the rhythm than in the previous test and this at first induced a slackening of the speed of addition, but later caused a very marked urge to overcome the tendency to slacken pace accompanying the slower rhythm, along with the general urge to do as well as in previous performances."

Because of this the rating of the development of intent is +2 and it is found that the 'score increase' is 2.78 times the s.d. Only one-fourth of 1 per cent. of the normal fluctuations are greater than this. The extra urge continued into the fourth test of the observation, along with the change in auditory stimulation. The increase in score was even greater, and was 3.78 times the s.d. The observer says in this connection:

"I am aware, or think I am aware, of a change in timing of the distracting accompaniment and so I am endeavouring to keep up my previous performance in spite of the tendency to slacken pace, which the slower rhythm seems to induce in me."

This illustrates the basis for the ratings of A's intent at the time of a change in the auditory stimulation, and the correlation of 0.70 implies that the changes in intent and in accomplishment are in close correspondence. The development of the special intents at the time of a change in the stimulation appears to follow the lines of the general intent of this observer.

This is also true for Observer B, and is best illustrated when the continuous change in auditory stimulation took place. This observer had developed a general intent to work at a comfortable pace, which was rationalized as her highest accomplishment. At the time of the continuous change (Observation VII) she reports:

"All the noise is more prominent, continuous repeated rings stand out. I feel a bit sick....All the noise is now continuous which makes me writhe inside."

The 'score change' is a decrease, which is 2.27 times the s.d. of normal fluctuations. A similar change in intent and decrease in score takes place upon the return to the original auditory stimulation in the tenth observation after the auditory stimulation had been eliminated from the situation in the ninth observation.

There are no significant changes in intent for Observer C throughout the experiment. Observer C was working with a strong general intent to accomplish more each test, and her results are in conformity with this intent. The auditory stimulation was not focal throughout the experiment. Changes in the auditory situation do not influence this general intent to any degree as indicated by the ratings. Five out of the eight are zero. Hence the correlation is of no value as a statistical measure.

Observer D's record is different. The changes in intent, accompanying the changes in auditory stimulation, follow the plan of the general intent to use every stimulus as an incentive to greater accomplishment. Changes both to the new auditory situations and back to the original ones were equally effective. The correlation between the changes in accomplishment

and intent at the time of changes in auditory stimulation is high for Observer D, but it is felt that it might have been higher if the estimates of changes in intent could have shown more accurately the degrees of strength of these specific urges.

These comparisons suggest two things:

(1) The dependence of any change in intent upon the general intent to work, and

(2) A general correspondence between changes in accomplishment and changes in intent.

VI. THE CONFIGURATIONAL INFLUENCE OF THE AUDITORY STIMULATION.

The use made of the original auditory pattern in the working situation is a question of experimental interest. Is a working configuration established which includes the auditory stimulation? The original auditory pattern was present during the first two observations and continued for the first two tests of Observation III when the change in the pattern of pitches was made by interchanging gongs 1 and 2 and gongs 4 and 5.

Observer A reports in the first test a "tendency, which is inhibited, to combine the work with the external noise and a strong tendency to overcome the bells." He says there is a "definite pattern of tones repeated." In the second test he is "not too keenly aware of any combination of factors that stand out as units." In the third test he says that "the combination of factors in bell tones is now much more marked. It has become quite a unit in the situation because of the usual sequence in the tones." In further description he says that the sequence of bells is the clearest part of the auditory situation. In the second observation he describes the "bell pattern as fairly marked on account of qualitative differences in pitch, intensity, and intervals between notes." In the second test of this observation we have this statement in retrospect: "In the beginning I tried to combine the rhythm with the adding process and when this failed, I tried to exclude the noise altogether." But this observer continues to report the "differences in intervals between noises, and the fact that any bell seems to cause a resonance in one or all of the others, and this tends to interfere with individual sounds." His awareness is similarly reported up to the time of the change in pattern in the third observation "of the qualitative unlikenesses of the individual bells, differences in intensity, and that they are linked together because of these qualitative differences in tone and the arrangement in sequence." He does not report any awareness of the change in pitch relations of the auditory situation when it takes place at the third test in the third observation. At the beginning of the fourth observation, one day later, however, this observer says that the auditory situation "seems to be a different pattern and not the same as I previously heard." In the second test of this observation he has "a feeling of doubt as to whether the bells are the same as before or different," and at the third test, when the change was back to the original situation,

he says, "I now think that the pattern is the same as before." At the fourth test he reports as follows: "I am now fairly sure that I have had the same pattern of bells all the time and that the idea that there was a new pattern was untrue."

Observer A was not influenced in his accomplishment by the change in auditory pattern (see Table III, column 1). There is no evidence of disturbance of awareness when a familiar rhythmic situation is changed. The change in accomplishment is similar to those changes made when the original auditory situation was present.

Observer B states in her report following the first test that her intent was "to add to the sound of the bells as rapidly as possible." The bells stand out as a unit in the second test; in the third it is reported that the "individual bells stand out less distinctly than before"; while in the fourth they are "more of a noise and undifferentiated now." "The bells stand out singly" in the second observation, but a little later this observer reports "don't notice separate bells so much now." It is "when they break their rhythm that I notice them, although I do not know if I am writing to their rhythm or not." Later it is reported that the bells are present as noises and "each single one of them stands out and there are no combinations. There is a time rhythm but no pitch rhythm for me." This is the situation at the introduction of the change in pattern at the third observation.

When the change in pitch relations of the gongs was made in the third observation, Observer B reports that she was aware of "pitches of bells and their order," which is the first time that these factors have been mentioned, but there is no awareness indicated of the change in pattern. However, there is a considerable increase in score, as shown in Table III. Six per cent. of normal fluctuations in score are greater than this increase.

Observer C reports in the first observation that she is "writing down figures in time with the bells, I think, and they make me feel that I must get on more quickly." She reports awareness of the "ascending sequence of the bells" and a "tune played by them." In the first test of the second observation she says, "I know which note is coming next," and in the second test there is the following report: "The tones are in the background and do not stand out apart from the task." In the third test it is reported that "at the beginning I wanted to write out of rhythm with the bells, I suppose more quickly than before, and this made it difficult." In the fourth test there is the statement that there are "no outstanding features, all merge into the general situation, although I am occasionally aware of the tone intervals, knowing what is coming next."

Observer C was not aware of the change in pattern when it came in the middle of the third observation. Accomplishment does not change for this observer, as indicated in Table III.

Observer D's results offer us an exception to the conclusion suggested

in the results of Observers A, B and C, that these observers are not co-ordinating their working rhythm and the auditory situation.

Observer D's awareness of the auditory situation was much more detailed and analytical than that of the other observers during the early observations. He reports in the first test that "some of the tones seemed to be going to make a tune and sustain a rhythm, when suddenly they are broken." In the second test he is aware of the motor and of "the monotony of the rhythm and the piercing qualities of the higher pitches," which were constantly present in awareness. Some relation was felt here between the accompaniment and motor co-ordinations. In the third test he is aware of various individual notes in the rhythm and in the fourth test the accompaniment is present as "noise as a whole which was much more blurred than before and no special attention was occupied with the various elements." In the second observation the accompaniment is spoken of as "a background of sound," and its effect as the "lulling monotony of tone rhythm." He says in the first test of the observation that "I made an adding rhythm for myself and this harmonized with the sound rhythm." Detailed descriptions of awareness of new factors in the accompaniment are given throughout Observation II. They are reported as demanding a certain amount of attention and impeding work, because they must be analysed. At this time "the rhythm is felt as a pattern as before, but also it is felt more strongly as a number of disjointed elements." In the fourth test of this observation "the aesthetic appeal of the monotonous tune has been lost and the new effects have not been built into a pattern of harmony. This greater complexity and brokenness of sound impedes work." This is the situation when the change in pattern takes place at the third test of the third observation. Then he reports a decided change in pitch and that "it was deeper and flatter than before and these new elements made the things noticed, such as rhythm, mixed harmonics, clangs, pauses, much more dimly noticed." He says that "certainly in the first part of the test it impeded work to a very considerable degree by inhibiting continuous effort and making the adding process follow from one sum to the next. After the minute's pause, however, the work proceeded with much more normal feeling of certainty."

The reports of Observer D suggest the possibility that this observer co-ordinated the work and the auditory pattern. They also suggest that this observer's awareness of the auditory situation was an extraneous awareness not connected with the working process. The change in auditory pattern might have disturbed the working rhythm or it might have caused the analysis of the auditory situation to become focal to the degree that the work was disturbed. Accomplishment decreased, but the decrease is not statistically reliable, as indicated in Table III. However, if the first half of the test (where the change in auditory pattern was made) is considered separately there is a significant decrease, which equals 4.83 times the s.d. of normal fluctuations for this observer.

The reports of all of the observers, up to the time of the change in pitch relations in the auditory situation, indicate that the possibility of co-ordinating the working rhythm and auditory rhythm occurred to all

of the observers and that the intent was developed in at least three out of four observers to effect such a co-ordination. But this intent soon changed to one to disregard the auditory situation as far as the task was concerned. Observer D seems to have developed a highly focal intent to analyse the auditory situation, particularly when the change in pitch relations of the pattern was made (Observation III). But with all observers the auditory situation is an extraneous awareness. The change in pitch relations does not influence the accomplishment of Observers A, B and C, and the accomplishment of D appears to be influenced because the intent to analyse the auditory stimulation becomes too focal, which is immediately corrected by greater awareness of the intent to do the task. The auditory situation, though present in awareness, is not a part of the working situation.

Upon the change back to the original auditory situation in test three of Observation IV there is one significant increase in accomplishment (see Table III), which again is for Observer D. Accomplishment was unaffected in Observers A, B and C. This raises the question again of whether or not the working rhythm of Observer D had not been co-ordinated in some way with the auditory stimulation. Of course, there was no exact timing, but there might have been a beneficial background of tonal pattern. This observation was repeated and a similar increase is recorded (see Observation IV, Table III, figures in parentheses). The ratios of the score changes in both observations, upon changing from the new auditory situation back to the original, are statistically reliable.

Is this increase in score caused by the return to the familiar auditory situation? The return to the original situation is not reported in the awareness of this observer. The "sound situation is far away and unimportant." But there is a tremendous development of the intent to work. After the third test of the fourth observation had begun the apparatus broke down. It was repaired and the test repeated. The observer reports, "I sat down with renewed zest and refreshed." For this reason the observation was repeated the following day. Again the auditory situation is reported as a background experience. The increase in score here came in the second half of this test, parallel to a development of the intent to work. The observer says:

"The main urge was to do something to ameliorate the unpleasantness experienced in the first part of the test, hence a more unusual effort to concentrate, and increased urge."

The increase in score in the first half of the test is only 1.46 times the s.d., while in the second half it is 5.63 times the s.d. Evidently the change in

intent in the second half of the test was responsible for the significant increase in score and not the return to the original auditory situation. A detailed description of the awareness of Observer D at this time gives us an understanding of what is taking place.

The observer is aware of the auditory pattern, but it is "not attended to—not noticed, as wall paper is not noticed after a month in a new room....I definitely felt myself faking up stimuli, by recalling past experiences in which persistence had been vital. By letting the 'sentiment' revived by these recollections play upon the present, new driving power was gained."

Observer D's search for incentives to stimulate him to increase his accomplishment is a vital part of his general intent to work and the auditory changes often provide the necessary incentives.

There is little evidence here or throughout the experiment that changes in the auditory situation have any influence upon the task except in so far as they are assimilated to an intention to make greater or less effort. There is no configurational influence in the strict sense of the word.

Table V. *Average accomplishment results with changes in auditory situation (summary).*

Observers				From original to new situation	From new to original situation	Both
For Observer A						
Intent development	...	...	...	0.5	0.5	0.5
Score change	...	...	...	- 6	0	- 3
Corrected score change	...	...	...	12	18	15
Ratio of change to s.d. of norm				0.66	1.03	0.84
For Observer B						
Intent development	...	...	...	0.3	- 0.8	- 0.3
Score change	...	...	...	10	- 12	- 1
Corrected score change	...	...	...	- 5	- 26	- 16
Ratio of change to s.d. of norm				- 0.17	- 0.85	- 0.51
For Observer C						
Intent development	...	...	...	1	0	0.5
Score change	...	...	...	28	19	23
Corrected score change	...	...	...	11	2	6
Ratio of change to s.d. of norm				0.34	0.06	0.20
For Observer D						
Intent development	...	...	...	1.3	1.5	1.4
Score change	...	...	...	30	70	75
Corrected score change	...	...	...	98	88	93
Ratio of change to s.d. of norm				3.99	3.62	3.80
For four Observers						
Intent development	...	...	...	0.8	0.3	0.5
Score change	...	...	...	28	19	24
Corrected score change	...	...	...	29	21	25
Ratio of change to s.d. of norm				1.21	0.97	1.09

Table V is prepared, as a summary of Table III, to assemble the results from the point of view that a familiar auditory situation might be more conducive to uniform accomplishment, or improved accomplishment, than a new auditory pattern or *Gestalt*, which might cause disorganization of the working process.

There is no evidence that a change in an auditory pattern is detrimental to accomplishment and return to an original or familiar pattern beneficial. For Observers A, B and C, the average changes in accomplishment at the time of change in stimulation are insignificant and well within the range of normal fluctuations. For Observer D these average changes in accomplishment are significant changes under both conditions and are approximately equal under both conditions. Also, the average change in accomplishment for all observers is about equal under both conditions—a change to the new auditory stimulation or a change back to the original auditory stimulation.

VII. THE INCENTIVE INFLUENCE OF THE AUDITORY STIMULATION.

This section deals with the special incentive effects of the changes in the auditory stimulation. Table III indicates that they were not great for Observers A, B and C, and considerable for Observer D. That is to say, none of the changes in accomplishment for Observers A, B and C, which came at the time of a change in auditory stimulation, were reliable statistically. But Observer A developed a special intent to increase effort to counteract any reduction in working speed which might be caused by the slower timing of the auditory stimulation in Observation V. The change in accomplishment is 2.78 times the s.d. of normal fluctuations.

Observer B developed a special intent to avoid the noise of the continuous change in auditory stimulation in Observation VII and there is a decrease 2.27 times the s.d. of normal fluctuations. This observer reports here that "when I attempt to do as much as I can, the bells become more conspicuous and more unpleasant." There is but one chance in a hundred that this decrease in score is due to normal causes. When the original auditory stimulation is returned at the third test of Observation X, after it had been eliminated in Observation IX, it is reported as a "hindrance which made me lose all desire to do as much as I can." The score decrease is 2.63 times the s.d. of normal fluctuations, which is greater than 99.6 per cent. of them. The auditory stimulation is an incentive to escape, for this observer, and the accomplishment is reduced where it becomes effective.

Observer C's reports indicate that the changes in auditory stimulation at Observations V, VII and IX had an incentive effect.

With the change in timing in Observation V she says the "bells are slower and louder. The task has become less pleasant and harder work. I was determined not to let this interfere with my work." With the continuous change in Observation VII she reports as follows: "Something very peculiar has happened to the bells. Seems as if I had gone to hell. At first this made writing difficult. Later I think it helped by stopping wandering thoughts." With the elimination of the auditory stimulation in Observation IX the report is as follows: "Silence stands out, rhythm of the apparatus is present. The cessation of the accompaniment hindered work and I determined not to let it interfere with my progress."

All changes in accomplishment at the time of these alterations in the auditory stimulation are increases which are greater than the normal. It is possible that the changes in auditory stimulation were slight urging factors for this observer, while for Observer B they tended to have the opposite effect.

Observer D is the one whose incentive was notably affected by the changes in auditory stimulation. Observer D, like Observer C, was working toward a goal of accomplishment, but unlike C, who intended to get there by development of method, D intended to get there by 'inspiration,' and he achieved his aim.

In the fourth observation the apparatus broke down at the beginning of the third test. When repaired this observer "sat down with renewed zest and refreshed." There was an increase in accomplishment 5.32 times the s.d. of normal fluctuations. This observer was always on the look-out for extraneous incentives. Elsewhere he says: "I definitely felt myself faking up stimuli, by recalling past experiences in which persistence had been vital. By letting the 'sentiment' revived by these recollections play upon the present, new driving power was gained." The change in timing of the auditory stimulation in Observation IV is reported as follows: "The slower pace was favourably reacted to as if it were something new. I was aware of a few spurts and then a lapse to a more effortless pace." There was an increase in score which was 2.58 times the s.d. The return to the original auditory stimulation is reported as follows: "The faster pace had a decidedly stimulating effect on the work. The pace of the bells made me increase and dragged me along with them. This soon gave place to an energetic state of mind in which I added very fast." The accomplishment increases 3.46 times the s.d. of normal fluctuations. This increase was maintained in the last test of the observation and is reported as a "considerable release of motor energy, which had been brought about by the faster pace of the bells in the third test and the result was that I tackled this test with great gusto." The general intent was to use every possible incentive to work "as rapidly as possible" and this procedure has its counterpart in the accomplishment.

The only suggestion in the experiment of unconscious motivation comes from the reports of Observer D at the continuous change in

auditory stimulation in Observation VII. This continuous change is reported

"As the greatest change in the sound situation since the beginning of the experiment. At first it seemed a mere chaos of cacophony and I said, 'Oh, my God,' and thought my ears would split. Then after having added a row or two, the constant buzzing noise became apparent. I concentrated on this and added like fury. Towards the end of the first part of the test it became apparent that the rest of the din was merely the old tune with a new noise. The magnitude of the disturbance then diminished a little and all interest in the sound situation as a problem vanished, and in the second part of the test it was merely present as analysed din." Upon starting the test, "I felt my mind brace itself and there was no feeling of continuity in the adding, but the imperative need was to keep adding without the slightest pause, the noise was so great that it deafened mental articulation and I proceeded in a mist of consciousness. Once or twice I thought I was to be reduced to making meaningless marks on the page and double effort was made to overcome this. The whole thing was reacted to as a test of endurance and it seemed to go on for an interminable time." The accomplishment increased 3.71 times the s.d. of normal fluctuations (Table III). In the next test of the observation, which included the same auditory stimulation, the increase was 7.42 times the s.d. This observer continues to analyse the accompaniment into its parts and reports that "the sound situation was not nearly such a strain as before, but its effect was to release a fund of energy, in the effort to combat the hostile situation, which resulted in unusual concentration and rapid work, and which produced intense feelings of mental strain as opposed to the physical, experienced in the previous test. I became hot, and developed pain in the head, a feeling that I was going to burst. [Observer was flushed and perspired freely.] The heightened activity was not voluntary, but absolutely determined by the stimulating conditions. I had to respond in this way. It was not until I was well into the second part of that test that the old idea of getting to the bottom of the page came to awareness and added its urge that this was an exceptional opportunity to achieve this end."

In the early tests of the eighth observation, next day, when the continuous changing auditory stimulation was present there was no such effect reported as described above, nor were the scores other than normal. The observer says in this connection that "the stimulating situation was much the same in its urging effect as the original one." Apparently the observer set off on this burst of speed as a result of the general intent to accept any extraneous stimulus as an incentive to greater effort.

The return to the original auditory stimulation in Observation VIII was accompanied by an increase in accomplishment 6.29 times the s.d. of normal fluctuations. All the time there are very few increases between control tests. The number of problems accomplished for control and experimental tests in Observations VII, VIII and IX are as follows for Observer D:

	VII		VIII		IX	
Control tests ...	(1) 884	(2) 861	(1) 876	(2) 896	(1) 897	(2) 902
Experimental tests	(3) 947	(4) 1051	(3) 1091	(4) 1011	(3) 1146	(4) 1027

The change in auditory stimulation came at the third test (test numbers are given in parentheses) and was continued in the fourth. This observer reports the change in auditory stimulation of Observation VIII as follows:

"It had a pleasant stimulating effect as if returning to the simple and familiar. It provided a harmonious rhythmical background, something simple and flowing and not occupying much of my attention, against which my internal drive to get to the bottom of the page could come up unaffected. I felt that the cacophonous experience had taught me to write faster."

The elimination of the auditory stimulation in Observation IX was accompanied by the greatest increase in score of all. The increase is 11.04 times the s.d. of normal fluctuations (Table III), which is an increase of 27 per cent. in amount of work done at a high level of achievement. In the fourth test the increase is continued to the extent of 5.67 times the s.d. Observer D reports these two tests as follows:

"The silence of the tune stood out like something positive. It was at first painful, then came an unusual release of motor energy that was tremendous and confused me. I had to make continuous markings on the paper at a great rate, and thought of the danger of being reduced to meaningless scribbling. The control over the released motor power increased with time, but the excitement was too great for controlled work. It seemed that this was the time to get to the bottom of the page and I must show myself that I could do it and risk making errors." In the next test, "The silence was still somewhat exciting, but the fear that the excitement would lead to errors produced an extra effort at accuracy at the cost of speed. The slowing-up was done deliberately in the hope that I would soon learn to be accurate as well as fast when experiencing a release of energy."

The general intent to get to the bottom of the page is very strong at this time, but in the first two tests of the tenth observation staleness is reported. Upon the return of the original auditory stimulation at the third test there is reported the "liberation of energy for a few lines, but I lost my sense of time and could not hurry, whereas I expected to be able to go really fast." There is a decrease in accomplishment of 0.38 times the s.d. Through a defective electrical contact the current was broken for a fraction of a second at the beginning of the second part (composed of 600 problems) of the last test. This caused a loud, long note to be sounded and held. Observer D reports that the sharp ring caused an awareness of doubling of speed of the auditory accompaniment and "I redoubled my own speed in a burst of exhilaration." The observer finished the test in the four minutes' time and had time over to "make at least a dozen corrections for illegibility." The increase was 7.79 the s.d. for this part and for the first part of the test it was 3.96 times the s.d.

Motivation evidently had increased for the last test, but the extraneous factor mentioned became an incentive to much greater effort in the second half of this test.

The results for Observer D have been reviewed in detail because this history of motivation gives a clear picture of the interplay of internal intent and external incentives in causing accomplishment. Indications from this experiment and particularly from this observer are that the general intent is the determining factor in the use of external incentives. They must be sought and accepted to be effective stimuli to increased effort.

VIII. COMPARISON OF INCENTIVE EFFECT OF VERBAL AND CONCRETE STIMULATION.

Verbal instructions are, of course, incentives when accepted, and it would be interesting to compare their effect upon accomplishment with the effect of concrete stimuli such as the auditory stimulation. The 'new intent' instruction to make a final effort to increase their score was administered to Observers A, B and C at the completion of the experiment. A comparison of the effect of the changes in auditory stimulation is shown in Table VI. These effects are in terms of the accomplishment increases over the S.D.'s of normal fluctuations for each of the four observers.

Table VI. *Comparison of accomplishment changes upon changes in auditory stimulation and upon change in verbal instructions.*

	Observers			
	A	B	C	D
Change in verbal instructions	+7.89	+7.13	+0.39	—
Change in auditory stimulation				
Highest	+2.78	-2.27	+0.84	+11.04
Average	+0.84	-0.52	+0.20	+3.80

That the increases in accomplishment are dependent upon the acceptance of the incentive has already been mentioned. The averages for the changes in auditory stimulation include times when there was probably little acceptance or development of intent to greater effort. Compared with the highest change in accomplishment at the time of a change in auditory stimulation there is evidence that the effect of a concrete incentive can be at least as great as a change in verbal instructions. But the former is not the usual way, in experimental work at least, to bring about a change of intent to work.

IX. SUGGESTED CONCLUSIONS.

This experiment was performed as an exploratory investigation in human motivation. Its report furnishes a genetic history of individual motivation over a considerable period of time in both its subjective and objective aspects. It turns out that the conscious aspect of the problem is emphasized in importance and the abstracted introspections offer valuable suggestions for further study.

Following the early historical interest in the introspective study of intent, or the 'intent to learn' as it was frequently referred to in the literature, psychologists have endeavoured to solve the problems of motivation by means of purely objective methods. This is necessary, of course, in the animal field. But investigators in human motivation for the past twenty years have continually referred to the inadequacy of these objective methods as usually used unaided. The results of experimentation bear testimony to this. From the writer's experimentation in this field comes the suggestion that thorough introspective reporting, related in time to reaction measures and stimulation, has much of positive value in the understanding of motivation. It may be that in the acceptance of incentives into the development of the intent there is a causative factor in motivation which is so slight in its physiological aspect that objective methods will be unable to measure it.

It is clear that the general intent of the worker determines the accomplishment and that this general intent is the resultant of past levels of motivation and what stimulation is accepted at the time the task is being performed. Any new stimulation, either concrete or verbal, may effect the intent, but it must be accepted as an incentive in order to become effective. Any stimulus may become an incentive either to greater or lesser effort, and the same stimulus may effect the accomplishment of two people differently.

Also, the intent determines the working situation. The configuration of stimulation is under direction of the intent. Environmental changes may cause awarenesses, but these awarenesses are not necessarily a part of the stimulating situation for the individual unless he intends them to be. The auditory stimulation was treated by different observers in different ways, but in general it only became a part of the working situation when accepted as an incentive. The intent separates off from the complex of received stimuli those which make up the working situation of the next moment.

The following conclusions are suggested:-

(1) The general intent to do a piece of repetitive mental work, such as was used in this experiment, is highly individual in nature, and its development throughout a period of work has also a high degree of individuality.

(2) Increases and decreases in accomplishment are closely correlated with any development of the intent to work.

(3) Specialized intents are often present, *e.g.* to allow extraneous thoughts free play in awareness, to slow up the work and allow extraneous thinking, to speed up the working pace so that it is focal in awareness, and to accept each change in conditions as a stimulus to a racing intent.

(4) Some individuals analyse the auditory stimulation very little and others in great detail, but both do so as a separate mental process to that of the task.

(5) While repetitive work may have a degree of automaticity, and high speed may be attained with little awareness, increases in speed and often the maintaining of a high speed seem to be accompanied by a fairly high degree of awareness of the intent.

(6) The development of the intent to increase speed may be accompanied by emotional disturbances which may cause a temporary decrease in accomplishment.

(7) Changes in objective conditions are accompanied by decreases, increases and no changes in accomplishment according to the development of intent of the worker.

(8) Objective changes, whether verbal or concrete, must first affect the intent before influencing the accomplishment. Changes in intent and accomplishment appear together, so this conclusion might be reversed. In the observations where the intent was reported in advance of work there is evidence that the change in intent is causative in the train of events.

(9) Concrete and verbal changes in stimulation may be equally effective as incentives, but their effectiveness is through a development of intent.

(10) Any configurational influence of stimulation appears to be under the guidance of the intent.

(11) A general intent guides the accomplishment. Specific intents are continually in development, are more focal in awareness, and re-define the general intent in relation to the task.

(Manuscript received 19 August, 1933.)

THE RÔLE OF GENERALIZATION IN LANGUAGE.

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- I. *Introduction* (pp. 434-6).
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I. INTRODUCTION.

THE use of language undoubtedly constitutes one of the most important and most frequent forms of human behaviour. Granting this, it is but natural that both psychologists and linguists have been interested in the interrelations of their sciences on this topic. Psychologists have used certain types of linguistic behaviour as examples of various psychological processes, such as abstraction, generalization, thinking, usually without specific attempts to explain the linguistic behaviour involved. Linguists have perhaps been more interested in psychological explanations of linguistic phenomena than have psychologists, but much of their work seems unsatisfactory from a psychologist's point of view. In this paper I propose to reconsider certain psychological explanations of linguistic processes which have been put forward. Obviously there is no need to explain all linguistic phenomena in terms of a single psychological process. Yet the processes of sound symbolism, word meaning, and analogic change, which have hitherto been treated as isolated phenomena, have a psychological similarity; all three of them are forms of the process of generalization.

Generalization may be defined as the transfer of a response from one situation in which it has been learned, to another in which it has not. The frequency with which this transfer will take place is a function of the similarity of the two stimulating situations. Whether the old response is appropriate in the new situation is irrelevant for our purpose. It is the process of transfer itself which is important.

Language has usually been defined in terms of the expression of ideas or the communication of thought. The popularity of such a definition can be largely attributed to the fact that language has generally been thought of as a purely human institution. But even so, such a definition is not logically demanded. Moreover, a definition of this kind tends to remove language from experimental investigation; the difficulty of attempting an experimental attack upon idea-expression or thought-communication is manifest. Recent writers have, however, discarded this point of view. De Laguna (4), pp. 9-19) and Gardiner (6), pp. 16ff.) have both substituted more objective definitions.

I propose to define language as a group of behavioural patterns (vocal, gestural, etc.) which, as a result of the community of learning among the users of that language, produce the same response in all of them. How nearly the same the response must be is a practical matter. Absolute identity is, of course, not meant¹. There is nothing in such a definition which precludes a wink or a blow from being classed as language, provided that all members of a given community have learned to give a common response to these stimuli. The fact that most language is vocal, while of tremendous importance in determining the detailed features of the language, is of absolutely no importance in a discussion of the fundamental nature of language. While the above definition sets no limits to the types of activity which might be classed as language, it does point out the fundamental characteristic of any activity which can be so classified.

A further important characteristic of language is that language develops, changes, and has an existence only as it is used by human beings. Even by competent linguists language has too often been discussed as if it had an existence of its own, had developed of its own accord, and had changed as a result of peculiar characteristics within its own structure, rather than of characteristics of its users. One example will suffice to illustrate this attitude. "Is there not a force," Aiken asks, "corresponding somewhat to inertia in physics, which to a large extent neutralizes and nullifies all the tendencies toward analogic change?" Answering this question in the affirmative, she decides to "recognize a linguistic principle of conservation, defining this as the influence exerted by the language itself to impede and resist change" (1), p. 55). It is of course the habit systems of the language users which resist change. Fortunately a growing recognition of this distinction is now leading many linguists to treat language as a type of human activity rather than

¹ See the section of this paper on "Word Meaning."

as a self-contained entity. Gardiner in particular has adopted this viewpoint, and the argument of his recent book (8) turns largely upon an examination of a particular example of speech in the concrete setting in which it occurred.

With an objective definition of language as a basis, the purpose of this paper is to define three specific linguistic phenomena (sound symbolism, word meaning, and analogic change) in terms of the more familiar process of generalization. While only three linguistic processes will be analysed here, it seems evident that others will likewise find a similar ultimate explanation. Two of these are the development of linguistic categories (too frequently ascribed to the action of a classificatory instinct) and types of sentence structure or word order.

II. SOUND SYMBOLISM.

It has frequently been asserted that a given sound has a fairly definite and unlearned meaning regardless of the word in which it occurs. For example, Jespersen (7) discusses in some detail the relations between sound and size, distance, states of mind, and so on. With respect to size Jespersen has pointed out that in many languages there is a significant relation between the particular vowel used and the size denotation of the word. Thus the vowel *i* (as it is found either in *pin* or in *feet*) occurs very frequently in words meant to designate that which is small, weak, insignificant, dainty, or refined; as in *little*, *petit*, *piccolo*, *wee*, *tiny* (which children often pronounce *teeny*), *slim*. The same vowel occurs in the names of small children and small animals, and is frequently used in endearing or depreciative words for children. *Child* (formerly with the *ee* sound), *kid*, *chit*, *imp*, *slip*, *pigmy*, *midge* are examples of such a usage. The same tendency is found in the names of small objects such as *tip*, *pin*, *chink*, *slit*. And lastly, it is found in the diminutive endings of such words as *Bobby*, *baby*, *auntie*, *birdie* (7), p. 402). The use of this vowel in words indicating smallness is not restricted to English. Jespersen quotes examples from Magyar, Latin, Greek, German, Danish, Dutch, Gothic, and Swiss German to illustrate the same usage.

Unlike other exponents of this general thesis, Sapir has experimentally investigated the relation between sound and size. In a study of vowel symbolism he made up lists of pairs of nonsense words differing only in one vowel. These lists were read to groups of subjects who were directed to differentiate between them on the basis of size. "For example, the meaningless words *mal* and *mil* were pronounced in that order and given the arbitrary meaning 'table.' The subject decided whether *mal*

seemed to symbolize a large or a small table as contrasted with the word *mil*" (9), p. 227). In general Sapir found that the so-called open vowels (those made with a wide opening between tongue and palate) were interpreted as referring to the larger of two objects, and the closed vowels (those made with a narrower opening between tongue and palate) to the smaller. Thus Jespersen's observations were experimentally confirmed. Again, we have such words as *large*, *ocean*, *vast*, *grand*, and *prairie* containing open vowels as opposed to the contrasting use of closed vowels illustrated in the previous list. But while it is easier to find examples which confirm Sapir's results than to find contradictory ones, it is not particularly difficult to find words such as *big* and *small* in which the vowels are not in agreement with Sapir's results¹.

In order to avoid the criticism that Sapir's results might be due to the language habits of the particular subjects used, the experiment should be repeated with subjects of as dissimilar linguistic backgrounds as possible. Sapir's group consisted mainly of high school and college students of American speech habits, although a few native Chinese were included.

The relation between vowel and size meaning might be due to something innate in those who first developed the language, and Sapir is inclined to give an explanation in nativistic terms, saying, "the tendency of symbolisms to constellate in accordance with an unconscious or intuitive logic...is not necessarily based on experience with the stimuli in their normal functional aspects....The influence of specific, functional language factors need not be invoked to explain these symbolisms" (9), pp. 238-9). It is, however, as unnecessary to postulate the existence of an innate basis for vowel symbolism as it is to postulate an innate basis, among English-speaking people, for the use of *s* as a plural sign, or, in another linguistic community, of some other specific instinct for forming plurals. This would necessitate the postulation of different instincts in each different linguistic community; further, it would mean that any particular child would find that language for which it has the proper instincts the one that could be learned with greatest ease. Such reasoning is completely out of step with current psychological theory. But one is not forced to accept the nativistic explanation of this phenomenon for want of a better one. The observed facts fit very nicely into an explana-

¹ A sample of words having a size reference picked at random (the first word at the top of every other page), from Funk and Wagnell's *Practical Standard Dictionary*, showed a ratio of about four to one of words confirming to words contradicting the general trend shown in Sapir's study.

tion based on the process of generalization. It is entirely possible that at an earlier stage the language contained more words of the type of *big*, *vista*, *sea*, and *immense*, as contrasted with *small*, *awl*, *auger*, or *tot*, *dot*, and so on. In these lists the names with a large size reference contain closed vowels, while those with a small size reference contain open vowels. If it is true that the language once contained a variety of such words and a variety of vowel-size relations (or what amounts to the same thing—no constant relation), then the present condition must have developed by the extension of the particular form which has now become fairly characteristic. But this is nothing more or less than the generalization of a particular relation which has largely supplanted all others. In such a case, as new words were added to the language (whether by borrowing, analogy, or intentional word-building) the vowel meaning would transfer to these new words; or those new words which conform to the already established trends would have the greater survival value. It is, of course, true that the generalization has not been complete, or it would be impossible to find words which contradict the general trend. It is also impossible to state just why this particular relationship rather than some other was the one which became generalized; but this limitation is characteristic of most explanations.

It must be granted that this explanation of sound symbolism is as purely theoretical and as little experimental as Sapir's nativistic one. However, it is more in line with current theory, and has the virtue of greater simplicity. Perhaps the question might be partially answered by contrasting the ease of learning sets of words which did or did not agree with such symbolism. For we might assume that if any innate basis for sound symbolism does exist, it would be easier to learn words which are congruent with this instinctive basis than to learn incongruent words. But it would be necessary to have subjects with no previous language habits. Since the experiment has never been performed, our explanation on the basis of generalization must remain merely a tentative one.

III. WORD MEANING.

The meaning of any word is in itself an example of generalization. Exactly the same response is never given twice, even by the same individual, to a given word, and yet the similarity of response is sufficiently great to enable us to say that the word has a fairly constant meaning. Bloomfield emphasizes this fact in stating that: "If each experience, owing to its indisputable individuality, were to be accompanied by a special utterance, no sound-sequence would ever be uttered more than

once, and communication by means of speech would be impossible. It is the habitual inclusion under one form of expression—that is, under one specific sound-sequence—of vast numbers of experiences presenting certain dominant features, which enables us to understand one another” (2), p. 83). Gardiner has expressed a similar view in saying “...every word without exception is a class-name; in uttering it the speaker is virtually saying, ‘Here is a class, and the thing I mean you to understand belongs to that class.’ The class is known to the listener by his *previous* experiences, the word having been applied by others or by himself to many other things falling under the same class” (6), p. 38).

The first factor determining the limits within which the response to any given word may vary is the nature of the individual’s previous experience; in general, the more experience, the less variability. For example, it is hard to predict what responses might not occur among ordinary auditors to the word *adiadochokinesis*. The variability of response among a group of neurologists would be very much less.

The second factor determining the variability of response to a given word is the proximity of similar or synonymous terms. Contrast, for instance, the generality of a *small nose* with the specificity of an *aquiline nose*. The latter term admits of less variability of response because there are more words describing the shape of noses, such as *Grecian*, *Roman*, *Negroid*, *straight*, *hooked*, *pug*, *broken*, *aquiline*, than there are words to describe their size, the latter category being almost restricted to *small*, *medium*, *large*, and *huge*. It is as if each variable, size and shape, included a given amount of territory. The more words there are describing a given variable, the less ground a particular one can cover; the more synonymous or similar terms there are to describe a given object or event, the more restricted the meaning of any one of them will become.

A similar illustration can be given in which form is the less specific variable. Except for the specific *square* and the less specific *Golden Section* there are no common names to designate particular types of rectangles. The one name *rectangle* is consequently very general in meaning and must of necessity cover all shapes and sizes of rectangles. However, the colour of the rectangle can be stated with a comparatively high degree of specificity because of the wealth of colour names. If we talk about a red rectangle we know its colour within a relatively narrow band of wave frequencies, but its actual shape may be almost anything. All that has been specified is that the figure has four right angles. The amount of variability of response to a given linguistic stimulus, together with measures of the interchangeability of two such stimuli, provides an

objective means for measuring the degree of similarity in the meaning of the two words.

The meaning of any word is ordinarily learned by encountering it in a number of similar situations, abstracting the elements common to these, and then upon this basis generalizing or applying the newly learned word in still other but similar situations. Our ability to learn the meaning of a word in any other fashion is solely due to the fact that language itself may be substituted for other types of activity. The process of learning the meaning of a word is then, ordinarily, one of generalization. Probably more than any other, this linguistic fact has been generally recognized by psychologists. (See for instance Dashiell (3), Chapter xv, or Weiss (12), Chapter XIII.)

IV. ANALOGIC CHANGE.

Probably the most clear-cut examples of the action of generalization in language are to be found in analogic formations, or, as they have frequently been called, proportional analogies. Such an instance is found in the process by which a child arrives at the form *gived* from the proportion:

talk : talked :: give : x

It is not to be understood that such a proportion is ever actually constructed, except as the linguist so constructs it as an illustration. What actually occurs is that the child has learned a great many verbs of the type *talk*, *talked*, or *want*, *wanted*; when occasion arises for him to use the past tense of a verb of which he knows only the present, the desired form may be obtained by generalization. The other types of analogic effects, which may be classed together under the heading of analogic change, are equally effects of generalization.

Bloomfield defines analogic change as any "change in the form of words [which] is in any sense due to their meaning" (2), p. 221). More commonly, analogic change has been defined as the change in phonetic form of any word due to interference of related or associated words. To illustrate: Latin *levis* (light) and *gravis* (heavy) would have become in Italian *leve* and *grave* if no influences other than the normal sound changes effective in the development of Italian had been acting. Actually the latter changed to *greve*, the *a* of Latin *gravis* being replaced by an *e*, thus increasing its phonetic similarity with its logical opposite *leve*. All cases of analogic change are similar to this example in that the change is in the direction of increasing the similarity to some related word or words. It is thus that analogic change is said to be the effect of the interference

of associated words; which is a more specific way of denoting the meaning relationship.

While meaning provides the essential basis for analogic change, a certain amount of phonetic similarity already existing between the related words is also necessary. Sturtevant writes that any association which is close enough to produce analogic change "must be based upon a certain degree of similarity between the words affected in respect both of meaning and of form. The word *mâle* could not have changed *femelle* into 'female' if the words had not already been similar in form" (10, p. 94).

These are the most important conditions necessary for analogic change. Its effect upon a language is another matter and ordinarily results in increased simplicity. English is a good example of a language in which analogy has simplified the declensions and conjugations and totally removed many former inflectional differences. While English has not yet reached the simplicity of an artificial language such as Esperanto, the trend has been in that direction. Actual attainment of this goal is constantly being checked by such restraining influences as written records; the speech of purists; the fact that complete learning of a given word form tends to prevent any alteration of that form; and the upsetting influence of general sound changes which cut across the lines of analogy and sometimes further alter words previously made similar by the action of analogy.

While both the conditions necessary for analogic changes and the simplifying trend of such changes have been fairly well recognized, explanations of this trend have taken several different forms. One of the most common tendencies has been to explain analogy in terms of purpose. Sometimes these explanations have posited a classificatory or systematizing instinct. Other explanations have made no mention of an instinctive basis but have been as clearly purposive in nature. Even as critical a thinker as Gardiner has fallen into this error, explaining that "analogy has as motive simply the desire for uniformity" (8, p. 140). Vendrys has formulated an equally teleological explanation in writing that "the need of uniformity is satisfied by recourse to analogy" (11, p. 156).

A second general form of explanation has been based on differences in the ease of pronunciation. Such explanations merely state that a difficult articulation is supplanted by an easy one. The elimination from English of the final sounds of German *ach* and *ich* has been explained on such grounds. While linguists do not generally adhere to such theories they crop up nevertheless. Thus Aiken explains all sound change as due

to the "unconscious urge toward ease in speech" (1), p. 123). Analogic effects Aiken treats as a special case of this unconscious urge.

Another type of explanation has been offered by some writers, notably Jespersen, in which these changes are attributed to the fact that as a given generation of children learn the language they do not learn it quite as did their parents. A number of generations of such slight differences will quite materially change the language. While Jespersen is undoubtedly correct in noting these differences, there is little evidence that they constitute a complete explanation of analogic change. Wolfe (14) has shown that in an experimental situation children are but little more prone to analogic changes than are adults. Using an artificial language technique (15), a group of children was taught a partially systematic language. Any changes introduced into the language by the children were carefully recorded. These changes were the same as those previously given by a group of adults (15), and occurred with but slightly greater frequency. Even if Jespersen's observation is correct, it still fails to answer the questions of why children do not learn the language as their parents speak it and of why these differences are in the uniform direction of increased simplicity.

The major criticism of all these attempted explanations is that they merely beg the question. This is a common criticism of purposive explanations, and applies here as elsewhere. The wholly unfounded assumption of a peculiar instinct that brings order into linguistic chaos is valueless. And little is gained by altering the purposive explanations to put them on to a non-instinctive level. As for 'ease' theories, they have frequently been criticized on the grounds that anything that is familiar is easy, and that consequently there is no basis in the relative difficulty of different sounds to explain sound change. The German *ch* is easy for a German, but an Englishman finds it difficult. Russians consider the English *h* a hard sound, but it gives English-speaking people no trouble. In short, the only criterion of difficulty is whether or not the sound is familiar. Theories based on the abstract *ease* of articulation of a given sound or sound sequence fail to take this fact into account.

Instead of explaining analogic change on the basis of a purpose, it seems more nearly correct to say that any analogic change is an example of the process of generalization. A form which has been well learned in one, or several, situations is generalized to cover other relatively similar situations. In learning English a child learns that many nouns form their plurals by the addition of *s*. Numerous actual examples are learned before the formal rule is ever encountered. When this plural-forming habit

is well established it continues to function in new situations. Thus the child may say *chocolates* without ever having learned or heard that specific plural; the plural *foots* may be similarly formed. Both of these examples serve to illustrate the extension of a well-established habit that has been built up in countless situations to cover unfamiliar but similar ones. Almost alone among linguists Moore seems to have recognized the importance of generalization in analogy. He writes: "It seems correct to say in general terms that when we make an analogy formation an established speech habit functions unconsciously in a situation which is similar to but not identical with situations in which it has functioned before" (8, p. 39). It seems preferable, therefore, to base our explanation of this phenomenon on the established habit systems of the language users rather than on a hypothetical instinct or directing purpose.

V. CONCLUSIONS.

The linguistic processes of sound symbolism, word meaning, and analogic change have been examined from the point of view of the psychological process of generalization. All three linguistic processes have been shown to be examples of this more fundamental psychological process. In attempting this demonstration it has been claimed that:

- (1) The widespread use of a given sound in words with a similar size reference is more probably to be explained on the basis of the spread or generalization of a few words in which such a relation existed, than on the ground of an innate feeling for sound symbolism.
- (2) The meaning of any word is a generalization of all the slightly different meanings given to it by the various members of the linguistic community, or by the same member at different times. In order to objectify the problems involved in an examination of this point, meaning has been defined in terms of the responses given to the word when used as a stimulus. Furthermore, the act of learning the meaning of a word is one of generalizing a number of varying situations in which the word has been encountered.

(3) Analogic change in its several different forms has been cited as one of the most obvious examples of the process of generalization, and that explanation has been substituted for purposive and 'ease' theory explanations.

Obviously then a thorough investigation of the psychological character and conditions of generalization is a matter of considerable linguistic importance.

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(Manuscript received 30 January, 1933.)

SOME ELEMENTARY TIME EXPERIENCES¹.

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THERE are many ways of studying the individual in all his different sides or aspects. One of these approaches is the behaviouristic—another, and perhaps a more fundamental one, is the introspective or descriptive. It is as a sort of old-fashioned Introspectionist I am going to speak.

William James has given us a famous instance of descriptive psychology in his chapter on “the stream of thought.” Regarding this “stream of thought” I shall try, on the basis of a great number of experiments, to make clear some points pertaining to our elementary time experience, and I shall confront them with our ordinary notions of time.

It is commonly held that a real event takes time, perhaps a very short time—a hundredth part of a second or less, but strictly instantaneous it cannot be.

If we have a sound stimulus in the outer physical world which has a duration—say of one second—we may say about our immediate sound experience, which is conditioned by this stimulus, that it has a duration—we experience duration. We are also able to say, which is a different thing, that we experience a beginning and an end to this sound. If we now make the stimulus very short the sound in immediate experience alters its time character profoundly. If we keep strictly to immediate experience and do not confound it with the sound stimulus, we no longer can differentiate between a definite beginning and an experienced end. It may be doubtful whether there is still just a trace of experienced duration or not. If, however, we make our experiment with a constant noise as a background, this doubt disappears. It is now perfectly sure that the sound experienced is both without experienced beginning or end and also without any experienced duration whatever. From a descriptive point of view it has strictly the character of instantaneousness².

¹ A paper read at the opening meeting of the Tenth International Congress of Psychology at Copenhagen, August 1932.

² Durup and Fessard have, in *l'Année psychologique* for 1930, studied this matter experimentally for visual and auditory stimuli. There is a certain analogy between these sounds without duration and the experienced visual lines and points without experienced breadth and extension respectively, studied in my “Visuell wahrgenommene Figuren,” I, (Kbh. 1921, Gyldendalske Boghandel, pp. 193ff.).

Benussi has, in Chap. 1 of his *Psychologie der Zeitauffassung* (Heidelberg, 1913), described an allied experience: if two sounds follow one another, we experience as a rule one sound before the other, with an interval between them. But if the two stimuli succeed one another with an interval of less than one-fifth of a second, the subject will experience two succeeding sounds, but he neither experiences any interval nor pause between them.

Let us now suppose that we have a succession of, say, three very short sound stimuli with very short intervals between them, and let us consider what would correspond to this in the "stream of thought." The three experienced sounds have no experienced duration, and between them there is no duration in the form of experienced intervals or pauses, nevertheless the whole has duration and there is experienced duration between sound No. 1 and sound No. 3. This seemingly paradoxical connection between experienced duration and elements without experienced duration is quite different from the connection between instant and duration in our ordinary notion of time.

Most psychologists know the classical complication experiment by Wundt: An indicator passes over the divisions of a dial. When it passes a certain division unknown to the subject a sound is produced. The subject is to decide with the help of his immediate experience at which division the sound occurred. His decision is generally erroneous. According to his immediate experience the sound coincides with quite a different position of the indicator from that which coincides with the sound stimulus in the physical world. Wundt was interested in investigating the laws and determining factors for these errors. We have quite a different interest in this experiment, but before expanding this point we wish to emphasize a fact, which is important if anybody wants to discuss what is put forward in this paper: The experiment is one among many instances, which we cannot mention here, which show that as a general rule time relations pertaining to the stimuli in the physical world cannot be transferred to the corresponding data of immediate experience in the stream of thought.

Wundt was interested in the relations between immediate experience and the stimulus situation; this is a psychophysical problem. Our interest is in immediate experience as such, and with regard to immediate experience there is a seemingly small and unimportant fact, which we have as yet not mentioned. In making the Wundt complication experiment, the subject has often some difficulty which is, however, soon overcome and forgotten. From his general notion of time, the subject knows,

or thinks he knows, that the sound must coincide with *one* position of the indicator; his immediate experiences are, however, quite different. He does not experience a coincidence between the sound and *one single* position of the indicator, but rather a coincidence between the sound and a somewhat broad, vaguely limited sector of the course of the indicator. The immediate experience is of such a nature that the subject is rather undetermined if he *has* to decide as to whether the sound is contemporary with the entire passage of the indicator through the whole sector or with an indefinite position of the indicator within the sector. We will call this peculiar experienced coincidence, where something which lasts a very short period coincides with something which takes a much longer time, the broad indefinite coincidence, by the way of giving it a name. This broad indefinite coincidence is an ordinary, very common experience. There is nothing peculiar about it in that way, and it is only when as introspective psychologists we try to get hold of it and describe it, that we detect the difference between this time relation and our general notion of time relations.

The subject who experiences the broad indefinite coincidence between the position of the indicator and sound, has also the experience that indicator and sound have nothing to do with each other. This 'irrelation' seems to be a condition for experiencing the broad indefinite coincidence. Conversely, when a sharp coincidence occurs in the stream of thought, it seems to have among its conditions an experienced relation between the two events. Personally, when I work with the complication experience, and desire a sharp coincidence, I find I succeed if I make myself some way believe that the indicator itself makes the sound in striking one of the divisions of the dial.

There are always a number of more or less seemingly independent events in the stream of thought, the mutual time relations of which are of the broad indefinite kind. The broad overlapping, experienced simultaneity between these different *parts* of the entire stream of thought is of the greatest importance for the experienced continuity of the *whole* stream of thought.

If two metronomes are made to strike, the one about 200, and the other about 210 times in a minute, the subject will as a rule have an experience of two different series of sounds, where each sound of the one series coincides in the broad indefinite way with some sounds of the other series. But in so far as the subject succeeds in experiencing the whole as only one series, then the time relations are sharp and definite. This characteristic difference regarding the coincidence (and related time

relations) between the experiencing of one or of two series has a certain bearing on the problem concerning the unity or the plurality of the contents of conscience.

Now comes my final point, where, although the differences are very important, what I have to say may seem to have some affinity with thoughts expressed by William James, W. Stern, Bergson, and Husserl. It is a fundamental and in reality a very complicated part of our general notion of time that there always is *one* absolute instant, which only for one instant has a peculiar character of being the present moment, dividing past and future time. Of two events or parts of an event which are not simultaneous only one can belong to the present moment. Let us suppose that we have two very short sound stimuli in the outer physical world, succeeding one another. If the first sound stimulus is present, the second one belongs to the future—the very near future—and when the second is present, the first belongs to the past—the very near past. If one of them is present, the other must be past or future. If we try to describe the immediate experience which two such sound stimuli condition, there is no difficulty when the interval between the stimuli is rather long—say one second. We experience a succession of two sounds. When the first is present the second belongs to the expected future. When the second is experienced as present, the first has the character of something past. But when the interval is very short, a fifth of a second for instance, we cannot describe our immediate experience without ridding ourselves of the ordinary notion of time which I have just mentioned. We experience a succession of two sounds—the one sound preceding the other, and the whole experience has duration. That is all right. The difficulties arise with regard to the character of future, past and present, especially the latter. Quite contrary to our general notion of time, the experience does not occur that one of the sounds is present and that the other belongs either to the just expected future or to the immediate past. Either both of them are past or both of them are future or both of them have the character of being present, although they are experienced as a succession.

This applies also to a rather short continuous noise whose stimulus belongs to the same order of time, although the noise is experienced as having duration and as having a beginning and an end. The whole noise has the character of being present. The same seems to apply generally to all sufficient small parts of the stream of thoughts and it probably is a fundamental characteristic of mental life.

The different discrepancies between our general notion of time and our immediate time experiences may perhaps be overcome through a

distinction between appearance and reality regarding our immediate time experience. But I fear that such a distinction may itself involve us in great difficulties.

Another way out is to alter our general notion of time in accordance with these data of our immediate time experience *which had not been taken account of when this notion was formed*. It is also possible to use both ways, introducing the distinction between appearance and reality to overcome some of the discrepancies and altering our general notion of time to overcome others. But one thing we must remember: it is nowhere proved that there is a way out and that we have not to leave the discrepancies as they are.

These theoretical questions are problems for the future, and it may be that it is not profitable to work with them before we get more facts. I think one ought to be cautious—more cautious than is often the case regarding theories.

In this connection a story about Herbert Spencer occurs to me, which also may serve as a sort of excuse for the 'small facts' which I have brought here. In an after-dinner speech he mentioned that he had once written a tragedy, after which Huxley arose and said he knew the theme of the tragedy, which was that of a big theory being killed by a tiny little fact.

(Manuscript received September 16, 1933.)

PUBLICATIONS RECENTLY RECEIVED.

A Hundred Years of Psychology. By J. C. FLUGEL. London: Duckworth and Co. 1933. Pp. 384. 15s. net.

Prof. Flugel begins his Preface by remarking unnecessarily: "Such a book as this is almost inevitably bad." During the last three or four years four histories of psychology have appeared in the English language. Only one of these is bad, and that certainly is not this one. Naturally any instructed reader who studies this volume will wish that some movements had been treated more fully and some less, and will not agree with all of the critical remarks, whether favourable or unfavourable. The book cannot seriously challenge comparison with the work of Gardner Murphy or more especially with Boring's History; but it does not pretend to do so. Both for the general reader and for the student not far from the beginning of his work, Prof. Flugel has produced a most stimulating and excellent treatise. He has written it in a concise and attractive manner, with full references to original works, with wide sympathy and with more than adequate knowledge. Considering how brief the expositions of the various points of view and detailed investigations are, the accuracy of this volume is astonishingly great. This history is in fact a fine achievement, with which the author has every reason to feel far greater satisfaction than is indicated in his opening remarks, and from which it is to be hoped large numbers of readers will obtain both stimulus and pleasure.

The Foundations of Psychology. By J. S. MOORE and HERBERT GURNEE. Princeton University Press. (London: Humphrey Milford.) 1933. Pp. xix + 287. 14s. 6d. net.

This book was first published in 1921 and was appreciatively reviewed in this *Journal* at that time. The need for a general theoretical survey of foundations is certainly not less now than it was then, and consequently the second and revised edition should be welcomed. The main changes are "a thorough revision of the sections on Behaviourism...; the addition of sections on the Gestalt Psychology or Configurationism, and on the Purposive Psychology of Prof. McDougall." The bibliography has also been revised. The new work has been done by Prof. Gurnee. In general the exposition is both clear and sound, and the criticisms are probably as fair as they could be without being greatly extended. The bibliography is almost entirely American.

Tratado de Psicología. Por W. RADECKI. Buenos Aires: J. Peuser. 1933. Pp. 428.

This general text-book follows traditional lines, dealing first with cognitive processes from sensing to the thinking processes, then with affective reactions, and last with conative processes. Two further sections deal with Social Psychology and with Industrial Psychology. An appendix discusses the place of psychology in the general scheme of the sciences.

Psycho-Analysis and its Derivatives. By H. CRICHTON MILLER. London: Thornton Butterworth, Ltd. The Home University Library. 1933. Pp. 256. 2s. 6d. net.

Chapter I of this book consists of 110 pages of exposition of psycho-analysis. Chapter II devotes 26 pages to criticisms of psycho-analysis. Chapter III studies "analytical psychology" (Jung) in 42 pages. These two systems are then contrasted in 18 pages. Adler is dealt with next, his assigned span being 28 pages. Chapter VI

briefly adumbrates the views of Hans Prinzhorn in 12 pages. A final very brief chapter compares all four and sums up: "If we attempt to appraise the relative merits of these systems of psychology we may safely begin by saying that Freud's is the most intricate, Jung's the most profound, Adler's the most practical and Prinzhorn's the broadest." Obviously and rightly Dr Crichton Miller has chosen to be the expositor rather than the critic in this book. His exposition is lively, very clear, sympathetic and about as correct as it could be made within limits as small as these. The volume is in fact a most excellent piece of work and ought to be widely read. A very good selection of books for additional reading is given. As a general introduction this book should hold the field for a long time.

The New Psychology and Religious Experience. By T. H. HUGHES. London: George Allen and Unwin, Ltd. 1933. Pp. 332. 10s. 6d. net.

Dr Hughes writes with so obvious a sincerity and honesty of purpose, and is so anxious to be scrupulously fair to the authorities with whom he disagrees, that his book can hardly fail to produce a marked and favourable effect even upon those readers who are unable to accept his point of view. His general purpose is to establish the reality and the vital importance of Religious Experience, and to show that any naturalistic account of it must be fallacious. Broadly, what he means by 'New' psychology is a dynamic rather than a static approach, but specifically he is mainly concerned to refute some of the views about religion which have been expressed by the psycho-analysts or by leaders of derived doctrines. His arguments are often lacking in subtlety, as, for example, when he says that because Freud is a thoroughgoing determinist he can find no place in his psychology for consciousness of sin. But yet again, as in what he says about the differences between religious experiences and certain analogous states produced by drugs, and in some parts of an extremely interesting discussion of conversion, Dr Hughes shows himself to be alive to differences that are often overlooked. The book is in fact worth reading. It is a product of long and careful study and thought. It is the expression of profound and genuine conviction. Though its value as argument differs considerably from part to part, as a whole it is a sincere effort to build upon the facts of religious experience as they are actually felt, and to show where in relation to them the most prominent of modern psychologists stand.

The Organism of the Mind. By G. RICHARD HEYER. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1933. Pp. xiii+271.

This is a translation, admirably executed by E. and C. Paul, of the author's *Der Organismus der Seele, eine Einführung in die Analytische Seelenheilkunde*. The book is an elaboration of lectures and still retains to a considerable degree the informality of the spoken word. There are two parts, the first dealing with organ neuroses and the second with psycho-therapeutic methods.

In the first part the author is largely concerned to show that mental and bodily states do not merely run on parallel lines, or act reciprocally one upon another, but are literally two phenomenal forms of one and the same 'life.' This he argues, in a most persuasive and attractive manner, giving numerous illustrations from his own practice and from a very wide range of literature.

In the second part, after two chapters of introduction on "The Attraction of the Unconscious," Dr Hoyer considers both sympathetically and critically the views and methods of Freud and Adler. He then proceeds to an exposition of the doctrines and practice of Jung which, difficult as it inevitably is, is very much clearer and more precise than anything that Jung himself has achieved.

An appendix contains a number of excellently reproduced drawings made by various patients, together with expository remarks thereon.

The book makes extremely good reading. It has wit, wide learning, and many flashes of insight. Whether its mystical interpretation of life is accepted or not, it succeeds in building up an impressive argument.

How the Mind Works. By CYRIL BURT, ERNEST JONES, EMANUEL MILLER and WILLIAM MOODIE. London: George Allen and Unwin, Ltd. 1933. Pp. 336. 7s. 6d. net.

Here are presented a series of broadcast 'talks' distributed as follows: *How the Mind Works in the Adult: The Conscious Mind*, by Cyril Burt; *The Unconscious Mind*, by Ernest Jones; *How the Mind Works in the Child: Problems in the Development of the Child*, by Emanuel Miller; *Problems in the Treatment of the Child*, by William Moodie; *How the Mind Works in Society*, by Cyril Burt. The volume will no doubt get, as it deserves, a wide and appreciative public. The different contributions are obviously presented very much as they were spoken, and here and there they are somewhat the monologues of a conversational schoolmaster who wishes to be very friendly but cannot altogether lay aside a certain superiority of attitude. Nevertheless, there is nothing that lacks interest, and though perhaps the psychologist who has already read and thought about the problems discussed will not find much new material, he will get both pleasure and instruction from the easy and clear exposition. Probably most readers will linger longest and with most satisfaction over Prof. Burt's discussions of the way in which the mind works in society. These are genuine social psychology and ought to stimulate many lines of further enquiry.

Hypnotism—the Friend of Man. By J. LOUIS ORTON. London: Thorson's. 1933. Pp. 280. 5s. net.

On the whole this is a good book. It avoids undue prolixity, it makes less extravagant claims for hypnotism than often are made, it gives some information of a historical order, and it describes working techniques of hypnotic procedure simply and clearly. Even so, a good many claims that it might be difficult to make good are advanced. The author is opposed to the exaggerations of phrenology but is himself attached to views concerning brain localization that run beyond available evidence. Also he tends to take symptoms that may be, simply as end results, indicative either of functional or of organic disturbance, and to urge that since these may sometimes be removed by hypnotic treatment, the treatment is adequate in many instances of organic disease. Further, as nearly always in this kind of book, there is no proper insistence on the distinction between the removal of symptoms and the production of genuine cures. The sections on sight and hearing do not show any close acquaintance with the physiology and psychology of the special senses. But if the book is read with due caution it will be found that within its short compass it gives information that will interest nearly everybody and be of considerable use to some.

Psycho-Analysis To-day: Its Scope and Function. Edited by SANDOR LORAND. London: George Allen and Unwin, Ltd. 1933. Pp. xv+370. 18s. net.

Here is published the following set of essays: *Freud's Influence on Medicine*, by S. Ferenczi; *Development of the Ego Psychology*, by F. Alexander; *Dream Mechanisms and Interpretations*, by M. A. Meyer; *The Theoretical Basis of Psycho-Analytic Therapy*, by H. Nunberg; *Character Formation and Psycho-Analysis*, by S. Lorand; *Development of Mental Hygiene*, by F. E. Williams; *Sexuality and its Role in the Neuroses*, by A. A. Brill; *Child-Parent Relationship*, by C. P. Oberndorf; *Understanding the Problem Child*, by L. T. Broadwin; *The Early Development of Conscience in the Child*, by M. Klein; *Prevention of Nervous and Mental Disorders in Childhood*, by T. H. Ames; *The Meaning of Neurosis and Psychosis*, by P. Schilder; *Pathological Character Formation: The Neurotic Character*, by E. Glover; *Hysterias and Phobias*, by A. Kardiner; *Obsessional Neuroses*, by B. D. Lewin; *Manic-Depressive Psychoses*, by G. Zilboorg; *Schizophrenias*, by R. LaForgue; *Paranoia*, by L. E. Hinsie; *Psycho-Analysis of Organic Psychoses*, by J. H. W. van Ophinsjen; *Psycho-Therapy and Psycho-Analysis*, by H. A. Bunker, junr.; *Psycho-Analysis and Internal Medicine*, by S. E. Jelliffe; *Psycho-Analysis and Anthropology*, by G. Roheim; *Psycho-Analysis and the Psychology of Religion*, by E. Jones;

Psycho-Analysis and Literature, by F. Wittels; *Psycho-Analysis and Criminology*, by P. Schilder.

From whatever point of view one approaches problems of human activity these essays form an impressive array of claims and achievements.

Mirror Writing. By MACDONALD CRITCHLEY. London: Kegan Paul, Trench, Trubner and Co., Ltd. Psyche Miniatures Medical Series. 1928. Pp. 80. 2s. 6d. net.

This essay contains a useful historical and critical account of mirror writing and related phenomena, together with a fair amount of clinical illustration.

The Structure of Insanity: A Study in Phylopathology. By TRIGANT BURROW. London: Kegan Paul, Trench, Trubner and Co., Ltd. Psyche Miniatures General Series. 1932. Pp. 80. 2s. 6d. net.

It is anything but easy to indicate briefly the argument of this book. The author's own concluding statement is: "In mental disease, whether it is represented in sensory repression, or in motor aggression, whether in the manifestation of clinical insanity or of social crime, whether in political strife or in industrial disturbances, there is need to abrogate mental and social images as therapeutic agencies for these disorders, and, laying aside opinions and ideas, to recognize the existence of morphological data represented in those physiological tensions and strains directly involved in the distortion of man's attention as a social process." This perhaps means that there are two sorts of 'attention,' the one kind directed upon whatever lies outside the bodily organism and the other upon whatever goes on inside the organism. To use the first kind of attention for the second class of phenomena can give no real insight. This appears to be somehow connected with a social determination of images and language. The exposition is however far from clear.

La Santé Physique et Morale par l'Autosuggestion. Consciente. Par ARMAND LEBRUN. Paris: Félix Alcan. 1933. Pp. 119.

M. Lebrun is a disciple of M. Coué. In this book he gives a brief but very enthusiastic account of Coué's work and of the theoretical notions which he considers to be involved in the doctrine of conscious autosuggestion. Many cases are reported.

Der Seelische Konflikt. Von STEPHAN KRAUSS. Stuttgart: Ferdinand Enke. 1933. S. viii + 126. RM. 8.80.

This is an essay of some value on the general theoretical side of psychology and considerably influenced by the work of Heidegger and Dilthey. The most detailed empirical part of the argument deals with conflict as it appeared in the life and work of St Augustine, of Amiel and of the Danish philosopher Kierkegaard. The character and significance of conflict are considered both in relation to the individual mental life and in various social settings. Conflict is regarded as both involving and expressing a degree of integration, and normally it has a positive and constructive function.

The Psychology of Effective Speaking. By T. H. PEAR. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1933. Pp. xiii + 232. 6s. net.

Prof. Pear, reacting from his attendance at the last International Congress of Psychology, has concluded that very many public speakers perform their task most inadequately and ineffectually. He does not, however, despair of public speaking, either as a form of entertainment or as a means of imparting information, but endeavours to lay down lines for its improvement. Unfortunately he does not distinguish efficiently clearly between the three different aspects of the subject: (1) purely

auditory effects produced by the pitch and intensity of the speaker's voice, and variations thereof; (2) the manner of verbal expression; (3) the matter which is expressed. Prof. Pear presumably intends his book to deal with the first two of these aspects, since a speaker deficient on the third count can only be advised to think again, or to stop speaking. Some useful advice is given with regard to the first two factors, but the book would be improved if this were more detailed and concrete, and tricked out with fewer irrelevancies. A plea that the speaking of English should be taught in the schools deserves attention, however.

Speech Disorders: A Psychological Study of the Various Defects of Speech. By SARAH M. STINCHFIELD. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1933. Pp. xii+341. 15s. net.

Although this is not a particularly well-written book, it contains an immense amount of information about recent researches into speech disorders. It is not, as many people might imagine, a text-book on aphasia. In fact aphasia in the technical sense occupies very little space in the volume. By far the most valuable part concerns the character, causation and treatment of speech defects in childhood. In spite of a rather exaggerated bent towards classifications and excessive technicalities, the author's account of early speech difficulties will be found of great value, in that it collects into a small compass the results of a mass of scattered work. The second part of the book deals with *Statistical Studies of the Speech of 3000 College Women and of Public School Groups*. These studies are based upon questionnaire results collected in America, and are interesting, though nothing very fundamental is claimed or attained. Other special topics dealt with are Handedness and Oral and Silent Reading.

Insanity as a Defence in Criminal Law. By H. WEIHOFEN. New York: The Commonwealth Fund. (London: Humphrey Milford.) 1933. Pp. xi+524.

First the book discusses tests of irresponsibility, indicating the rules followed by courts at one time or another, and stating the exact number of jurisdictions supporting each rule discussed. The same general plan of exposition, citing numbers of jurisdictions, is adopted as regards each major problem discussed: burden of proof; witnesses and their testimony; pleading and procedure; and present insanity at the time of criminal procedure. The author sets himself "to find and read all the American cases on the subject that have been reported, as well as the leading and representative English cases." The book is thus weighted with a tremendous mass of material, but nevertheless the author has continued to keep his argument well in hand, and to present a clear picture of the present state of affairs. A final and rather radical chapter on suggested fundamental reforms brings this distinctly valuable book to an end.

Have You Good Taste? By MARGARET H. BULLEY. London: Methuen and Co. 1933. Pp. vii+52. 3s. 6d. net.

This extremely attractive small book deals with "the training of taste in regard to the lesser arts." The arts considered are: woodwork, metal work, ceramics, glass, textiles and printed papers. First comes a well written and clear essay by Miss Bulley, wherein the response to 'harmony' is treated as the key to good taste. Then come a series of finely reproduced photographs of characteristic objects in the arts concerned, which the reader is invited to class as 'good' or 'bad.' Critical notes follow for the guidance of any reader who has conducted the experiment, and two appendices, one entitled *Train your Taste*, and the other, by Prof. Cyril Burt, entitled *A Test in Aesthetics*, conclude an unusually delightful and useful volume.

Modern Educational Psychology. By B. N. JHA. Allahabad: The Indian Press, Ltd. 1933. Pp. xv+448. 6s. net.

This is a clear and interesting text-book, very pleasantly produced. It is as accurate as anybody could reasonably expect within the limits of its space, and though it cannot claim much originality either in outlook or in method of treatment it is undeniably well informed, and could be used with success as an introduction to larger or more specialized works. The author deals first with general questions of definition and method, then with heredity and environment, and with the more active overt forms and determinants of conduct. He discusses next, in the regular order, sensation and perception, attention, memory and association, imagination, thinking and reasoning. Final chapters deal with learning and habits, intelligence testing and adolescence. Throughout, the significance of the problems involved from the point of view of education in the school sense is kept well to the fore. This is undoubtedly a useful introductory text-book of psychology for the average teacher.

Le Jugement Moral chez l'Enfant. Par JEAN PIAGET. Paris: Félix Alcan. 1932. Pp. xi+428. 60 fr.

Prof. Piaget here carries his fruitful conversational method into the sphere of the basis of respect for and understanding of regulative rules of conduct. He starts by studying rules in play, and proceeds to a consideration of the attitudes of children towards rules imposed by older people, taking for particular discussion attitudes towards lying. Next comes a careful treatment of the development of the notion of justice, and of the practice of social co-operation. Finally, there is an exposition and criticism of the views of other writers, in particular those of Durkheim, Bovet and Baldwin, and a statement of Piaget's own conclusions. In general Piaget, as in all his other discussions of the developing activities of the child, starts from the position that the young child is excessively ego-centric. At this stage all social rules have an aspect of external compulsion. In some respects or in some cases this stage may never be passed. Normally, however, developing intelligence and sentiments lead to the notion and practice of justice and social co-operation. Moral rules now lose a great deal of their external constraint, and become absorbed into the personality, the latter being in the process very largely socialized. Both for its realistic methods and for the clear and precise manner in which the argument is developed, this book is a notable contribution to its topic.

Handbook of Child Psychology. Edited by CARL MURCHISON. Worcester, U.S.A.: Clark University Press. (London: Humphrey Milford.) 1933. Pp. xii+956. 24s. 6d. net.

The second edition of this 'handbook' (a strong hand is required to hold it, and a strong mind to take it all in) contains several new contributions besides most of the original ones in a revised form. The entirely new material is as follows: *Origin and Prenatal Growth of Behaviour*, by L. Carmichael; *The Neonate*, by K. C. Pratt; *Maturation and the Patterning of Behaviour*, by A. Gesell; *Locomotor and Visual-Manual Functions in the First Two Years*, by M. M. Shirley; *The Measurement of Mental Growth*, by F. L. Goodenough; *Sex Differences*, by B. L. Wellman; *Speech Pathology*, by L. E. Travis; *The Physiological Appetites*, by W. E. Blatz; *The Child with Difficulties of Adjustment*, by P. Blanchard; *The Adolescent Child*, by L. S. Hollingworth. The whole bears witness to the extraordinary vigour with which research into the activities of young children is now being carried out, particularly in America. The handbook demands and should receive a place in every psychological library. It must remain, at any rate for a year or two, the standard manual of a consultation upon any of the topics with which it deals.

La Mesure de l'Intelligence chez les Écoliers. Par MARCEL FOUCAULT. Paris: Delagrave. 1933. Pp. 136. 15 fr.

The author pays an enthusiastic tribute to the pioneer work of Binet and Simon but points out that extension is called for. Particularly desirable are methods of measuring the intelligence of students above the age of ten years, a way of distinguishing clearly between development proper and aptitude, and a more precise notion of intelligence. The book gives an account of his endeavour to fill up these deficiencies. His Five Tests and the general results of their application are given in full, and his discussion is throughout concise and clear. The methods themselves do not contain much that is original, and it does not seem that the author has a very close acquaintance with modern methods of statistics, but his general discussion contains much that is worth careful study. His insistence upon training in the way the tests are to be given is a good point overlooked more often than should be the case.

La Psychologie Bergsonienne. Par ROGER E. LACOMBE. Paris: Félix Alcan. 1933. Pp. 325.

Many readers will remember the extraordinary vogue which Bergson's works had in the years preceding the war. All these will find this admirable critical study full of interest, and even those who cannot remember how people were swept away by the brilliant French philosopher will, if they care to turn over these pages, thoroughly enjoy a sympathetic, accurate and penetrating piece of criticism. Prof. Lacombe considers successively Bergson's views about memory, perception, duration, freedom and creativeness, and intelligence and intuition. His expositions are concise but just. His criticisms, particularly as they are stated in the concluding chapter, are pointed and relevant. He pays, as everybody must, a warm tribute to Bergson's originality and to his great critical powers. But he is rightly suspicious of a "*doctrine qui ne veut être vérifiée que par un recours à une expérience intérieure ineffable*." The main criticisms are that Bergson regularly over-simplifies the hypotheses which he opposes and then renders them an easy prey to ridicule; and that carried away by enthusiasm he often takes a conclusion that has been arrived at by dialectic as the direct expression of a group of facts. But this book is not destructive merely. All the time it tries honestly to show what is valuable in the views which it criticises, and how this might be developed in more fruitful ways than Bergson himself adopted.

Lehrbuch für den Philosophischen Einführungsunterricht an österreichischen Mittelschulen. Von JOSEPH KRUG und OTTO POMMER. Erster Teil: *Psychologie*. Wien: Hölder-Pichler-Tempsky. S. ii+122. M. 2.80.

This short handbook contains three divisions: *Das Seelenleben und seine Erforschung*; *Das Seelenleben in seinen Einzeltatsachen*; *Aus den Sondergebieten der Psychologie*. It will be realized that a very large ground has to be covered in a short space, and though in general the exposition is clear, it is doubtful whether any reader who is not already fairly well instructed would gain much from a study of this book.

Kritik der Sogenannten Praktischen Erkenntnis. Von ALF. ROSS. Kopenhagen: Levin and Munksgaard. 1933. S. 456.

This study of morals is mainly devoted to an expository and critical survey of the well-known moralists of this country and of Germany and France. The author has studied very widely indeed and includes in his scope many writings which would usually be considered of psychological import and significance only. His standpoint is philosophical but neither metaphysical nor intellectualistic.

The Structure of our Apprehension of Reality. By C. LAMBEK. London: Williams and Norgate, Ltd. 1933. Pp. 95. 4s. 6d. net.

COMMITTEE FOR RESEARCH IN EDUCATION.

The following is a summary of the replies received to the Committee's enquiry with regard to research in educational psychology planned and in progress during 1933-4.

BIRMINGHAM. SALTLEY COLLEGE.

An Enquiry into the Responses of Children aged 11-14 to Verse.

UNIVERSITY OF BIRMINGHAM. EDUCATION DEPARTMENT.

1. Tests of Higher Mental Processes.
2. An Enquiry into Tests of Specific Abilities needed for Secondary School Work.
3. Some Aspects of the Training of Teachers Abroad.
4. Studies in the Psychology of Early Childhood.
5. An Experimental Enquiry into the suitability of the Otis Group Tests of Intelligence for use with Matriculated Students.
6. Preferences of Children and Adults in Poetry and Prose Literature.
7. Correlation between Performance in English Composition and Intelligence Tests.
8. Errors in English Composition as revealed in Essays written by High School Certificate Candidates and University Post-Graduate Students.
9. Sex differences of five-year-old Children in dealing with Spatial Relations.
10. Formal Training: a Historical and Critical Review with a Re-examination in the light of Modern Psychology.
11. The Motives of English State Education, considered in relation to the Problems of Educational Equality.
12. The Attention Values of certain forms of Public Appeal.
13. Problems of Colonial Education.
14. The Development of the Appreciation of Art in Children and Adults.

BRIGHTON. THE MUNICIPAL TRAINING COLLEGE.

1. Investigation into Racial Prejudice among Training College Students (in connection with the Psychological Section of The Training College Association).
2. The Psychology of Study as applied to the needs of Training College Students.

COLCHESTER. ROYAL EASTERN COUNTIES INSTITUTION.

1. Mongolian Imbecility.
2. Relationship between Head Size and Intelligence.
3. Intelligence of Children of Certified Mentally Defective Parents.
4. Study of Simple Primary Aments.

CREWE. CHESHIRE COUNTY TRAINING COLLEGE.

An Enquiry into the Origin and Growth of Sentiments.

UNIVERSITY OF EDINBURGH. DEPARTMENT OF EDUCATION.

1. Oral Group Tests of Intelligence and Achievement for Wide Age-ranges.
2. Continued work on the Moray House Group Tests of Intelligence and Achievement for eleven-year-olds.
 - (a) Answer patterns.
 - (b) Norms.
 - (c) Age allowances at different levels.
3. Theoretical Research into the Interpretation of Matrices of Correlation Coefficients.

UNIVERSITY OF GLASGOW. THE PSYCHOLOGY DEPARTMENT.

1. Appreciation of Single Colours by Young Children.
2. The Existence of a Group Factor in Performance Tests.
3. Character Training in School Life.
4. Phenomenal Regression in Childhood.
5. The Educational Value of Colour in Pictures.
6. Factors in Musical Ability.
7. The Nature of the Learning Process in the Post-qualifying Stage.
8. The Use of Performance and of Verbal Tests in the Diagnosis of Mental Deficiency.
9. Literary Composition and its Component Parts.
10. Composition Testing.
11. Poetic Appreciation in Children.
12. Art Judgment—its correlation with *G*.
13. The Effect of Training in the Principles of Composition on the Results of the Art-judgment Test.
14. Mechanical and Reasoning Abilities in Arithmetic.
15. Dependence of Visual Remembering on Verbalization.
16. Saturation of Form-relations Test with *G*.
17. Bilingualism.
18. The Process of Geometrical Deduction.
19. Late Qualification for the Post-primary Course.
20. The Construction of a Perceptual Test of Intelligence.
21. The Problem of Children's Stealing.
22. Manipulative Reactions of Young Children.

HULL. THE MUNICIPAL TRAINING COLLEGE.

1. Part played by Imagery in Learning Poetry by Heart and in Literary Appreciation.
2. Individual Child Study of Normal Children. Special attention to Factors of Home and Social Environment. All studies based on personal observation of children.
3. Selective Studies of Backward Children. Grading of work with special reference to individual capacity. Detailed records.
4. Studies in Vocational Preferences of Adolescent Girls. Studies in Children's Interests.

5. Investigation of "Racial Consciousness in Children, Adolescents, Adults."
(Continued and extended; in connection with the Psychological Section of the Training College Association.)
6. Study in Musical Broadcasting.

HULL. THE TRAINING COLLEGE, BEVERLEY ROAD.

1. The Child's Power of Learning by Visual Imagery. (Third Series.)
2. Colour Preference in Children between the ages of 9 and 11. (Third Series.)
3. The Relation of Attention, Interest and Association to the Child's choice of Methods of Learning.

UNIVERSITY OF LEEDS. DEPARTMENT OF EDUCATION.

1. The Vocabulary Burden in the First Year of French.
2. Children's Errors in French Composition at the School Certificate Examination.
3. Backwardness in Arithmetic.
4. Plateaus in Motor Learning.
5. Secondary School Examinations in Physics.
6. The Validity of some Non-Verbal Tests of Intelligence.
7. Mathematical Ability with particular reference to School Geometry.
8. An Experimental Study of Retentivity.
9. The Conceptual Development of Adolescents.
10. The Reliability of Entrance Scholarship Examinations into Secondary Schools.
11. The Intelligence of School Children of some Castes in South India.

UNIVERSITY OF LONDON. BEDFORD COLLEGE.

1. Some Factors Involved in the Process of Problem Solution with special reference to the Concept of Insight.
2. Emotional Development in Young Children.
3. Temperamental Differences in Young Children.
4. The Role of Organization in Learning.
5. The Influence of Incentives on Learning.

LONDON. EAST LONDON CHILD GUIDANCE CLINIC.

1. Relationship between Social Conditions and Neurosis in Children.
2. Relationship of Intelligence and Delinquency.
3. Temperamental Difficulties in Relation to Behaviour Disorders.
4. The Rorschach Test applied to Children (in connection with Manchester University).
5. Causes of Enuresis.

UNIVERSITY OF LONDON. GOLDSMITH'S COLLEGE.

An Investigation into the Causes of Specific Backwardness in Composition amongst Elementary School Children.

UNIVERSITY OF LONDON. INSTITUTE OF EDUCATION.

1. Prognostic Tests of Ability in School Geometry.
2. Prognostic Tests of Language Ability.

3. An Estimation of the Effects of Evening Institute Work on the Social Attitudes of Girls.
4. The Reliability of Examinations for Entrance to Secondary Schools.
5. Prognostic Tests of Ability in Advanced Needlework.
6. Transfer of Training from the Inculcation of Scientific Method in the Science Classroom.
7. The Psychology of Reasoning and the Mathematical Concept of Order.
8. The Psychological Problems of Culture Contacts (South Africa).
9. The Measurement of the Child's Social Attitudes.
10. The Logical Basis of Intelligence Tests.
11. The Testing of Special Aptitudes for certain School Subjects.
12. The Scientific Interests of Non-Science Pupils of School Leaving Age.
13. Tests of Literary Appreciation.
14. The Effect of Speech Training on Oral and Written Expression in the Mother Tongue.
15. An Enquiry into the Geometrical Concepts of Young Children.
16. The Effect of Self-Criticism on the Pupil's Appreciation of Art.
17. Studies in Teaching Methods: a Comparison between Group and Individual Methods in various School Subjects.
18. A Comparative Study of Behaviour Problems in Schools which emphasize either Freedom or Discipline.
19. The Relation between Interest and Achievement in Learning Modern Languages.
20. The Relation of the Child's Imagery to his Ability in Secondary School Mathematics.
21. The Child's Ability to remember Common Objects and their Characteristics.
22. The Development of Certain Character Qualities in Senior School Pupils by Deliberate Methods, and the Possibility of Measuring such Development.
23. A Study of Literary Appreciation and its Relation to Imagery.
24. A Standardization of the Merrill-Palmer Tests for English Children.

UNIVERSITY OF LONDON. KING'S COLLEGE.

1. The Automatization of Voluntary Movement.
2. The Influence of Relations involved in Learning.
3. The Influence of Time-intervals on Recall.
4. On the Introspective Method.
5. The Influence of Volition on Retentivity.
6. Aesthetic Appreciation.
7. Blood Distribution during Volitional Activity.
8. The Influence of Volition upon Thinking.
9. The Causes of Faulty Speech and Writing in Adolescent Boys.
10. Suggestibility and Character.
11. Perseverance and Perseveration.
12. The Nature of Incentives and Motives.
13. Volitional Attitudes.
14. The Influence of Volition and Affection on Ergographic Work.
15. Investigation of Constant Errors.
16. An Investigation into 'g', 'p' and 'w' of Mentally Defective Children.

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UNIVERSITY OF LONDON. UNIVERSITY COLLEGE.

1. Intelligence Tests for Use in Child Guidance Clinics and Mental Hospitals.
2. Some Problems in the Psychology of Aesthetic Appreciation.
3. Psycho-Galvanic Phenomena during free Association.
4. Psychological Characteristics of Retarded Children.
5. The Temperamental Characteristics of Retarded Children.
6. Non-Verbal Characteristics of Anglo-Chinese Children.
7. An Analogies Test with Visual Material.
8. Spatial Perception as a Special Factor.
9. The Development of Language in the Child.
10. Tests of Perseveration (*p*) and their Relation to Schizophrenia.
11. The Relation of Introversion and Extroversion to the Factors of Perseveration (*p*), Stability (*w*), and Fluency (*f*).
12. The Influence of Temperature and Ventilation on Mental Work.
13. The Measurement of Speed and Power by the C.A.V.D. Tests.
14. Perceptual Tests in Visual, Auditory and Tactile Material, with special reference to their Value as Tests of Intelligence.
15. Psychological Factors in the Choice and Appreciation of Dress.
16. The Factor Analysis of Perceptual Tests.
17. The Factorization of Reliability Coefficients.
18. The Measurement of Perseveration (*p*), Stability (*w*), and Fluency (*f*), with special reference to Unresolved Mental Tension.
19. A Study of Emotional Display and its Relation to Known Orectic Factors.
20. The Play of a Cretinoid Child.
21. Certain Problems in the Psychology of Religion.
22. Dextralities of Manual Characters.
23. Suggestibility and its Relation to Social Problems.
24. A Study of the Crying of Infants in Relation to the Feeding Situation.
25. A Study of the Psychological Factors in Learning Control of the Sphincters in the first three years.
26. A Study of Children's Drawings in the first six years.
27. The Relation between the Aggressive and Sexual Instincts as shown in early Development.
28. A Study of the Emotions in Infants and Young Children.
29. A Study of Logical Processes in Young Children.
30. The Development of Motor Co-ordination in Children between two and three-and-a-half years.

LONDON. NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

1. Standardization of an alternative Version of the Institute's Intelligence Test.
2. Analysis of Qualities and Abilities required for Secretarial Work, Nursing and Teaching (*continuation*).
3. Experiments in Vocational Guidance in Birmingham, Willesden, Fife and Borstal (*continuing*).

UNIVERSITY OF MANCHESTER. DEPARTMENT OF PSYCHOLOGY.

1. Speaking as an Expression of Personality.
2. Differences between Spoken and Written Style.

3. Art-Forms in Speaking.
4. Attempt to produce Synaesthesia experimentally.
5. Experiments on "Electrical Taste."
6. Factors in Auditory Localization.
7. The Development of Intelligence.
8. Introverted Tendencies in Shy Children.
9. The Rorschach Test applied to Children.
10. The Spontaneous Drawings of Children with special reference to Remote Tendencies.
11. The Relation between Intelligence and Mathematical Intelligence.
12. The Appearance of Colours.

NOTTINGHAM. UNIVERSITY COLLEGE.

1. The Development of Language in Young Children.
2. A Test of the Two Factor Theory.
3. A Survey of the Education of the County.

UNIVERSITY OF SHEFFIELD.

1. Analysis of the Learning Curve.
2. Development of Moral Values among Senior School Pupils.
3. The Visual Factor in Methods of Teaching Geography.
4. The Bearing of Differences in Colour Sensitivity upon the Problem of Learning under School Conditions.
5. Mental Ability and Correlation Coefficients.
6. Children's Vocabularies.

SOUTHAMPTON. UNIVERSITY COLLEGE.

1. Concept of Law and Order in Science.
2. Development of Imagination and Conceptual Powers in Children.

UNIVERSITY OF SOUTH WALES. CARDIFF.

1. Use of Intelligence Tests in Secondary School Admission Examinations (*continuation*).
2. Psychology of Artistic Creation with particular reference to Literature.
3. Literary Appreciation.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

- December 9, 1933. Annual General Meeting.
- Papers: "The Concept of Emotion in Relation to the Theory of Temperament." By C. A. MACE, M.A.
"A Study of the Gestalt Problems in Interrupted and Uninterrupted Tasks." By A. R. PACHAURI, M.A.
"The Significance of Fluctuations of Attention." By S. J. F. PHILPOTT, D.Sc.
- Demonstrations: Apparatus for the Comparison of Psycho-analytic Interpretations with Psycho-galvanic Deflections. By Dr DENNIS CARROLL and J. MALCOLM FORBES, S.B., M.A.
(a) An Adaptation of Seashore's Audiometer; (b) An Adaptation of Marbe's Colour Variator; (c) An Electric Dotting Machine; (d) A Phonic Chronometer. By J. T. RAPER.
- February 14, 1934. Discussion on "The Denotation of the Term 'Sense-Perception' in Psychology." Opened by Prof. B. EDGEELL.

LECTURES.

- January 26, 1934. "Work and the Worker." By Dr MILLAIS CULPIN.
- February 23, 1934. "The Psychology of English Composition." By Dr P. B. BALLARD.

SECTIONAL MEETINGS.

MEDICAL.

- November 22, 1933. "The Primitive Catatonic Psychosis of Idiocy." By Dr C. J. C. EARL.
- December 13, 1933. Symposium: "The Psychology of Peace and War." By Dr EDWARD GLOVER and Prof. MAURICE GINSBERG; discussion opened by Dr JOHN RICKMAN.
- January 24, 1934. Address from the Chair: "On Quacks and Quackery." By Dr JOHN RICKMAN.
- February 28, 1934. "Non-Sexual Factors in the Evolution of Culture and Character." By Dr IAN D. SUTTER.

EDUCATION.

- December 4, 1933. "Is a 'Science of Education' Possible?" By Mr E. R. HAMILTON, M.A., B.Sc.
- January 4, 1934. "Popular Education and Social Responsibility." By Prof. F. C. BARTLETT.
- February 5, 1934. "Methods of Re-Building Character in Young Delinquents." By Dr H. E. FIELD.

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INDUSTRIAL.

December 7, 1933. "Our Predecessors." By Dr MAY SMITH.

January 4, 1934. Discussion on "Perseveration in Industry." Opened by Dr W. J. PINARD and Dr W. STEPHENSON.

February 15, 1934. "The Worker's Attitude towards Industrial Psychology." By Mr J. H. MITCHELL.

AESTHETICS.

December 8, 1933. "Sur-Realism." By Miss V. BURDWOOD EVANS.

February 9, 1934. Discussion on "Is Beauty Purely a Human and a Subjective Quality?" Opened by Dr C. S. MYERS.